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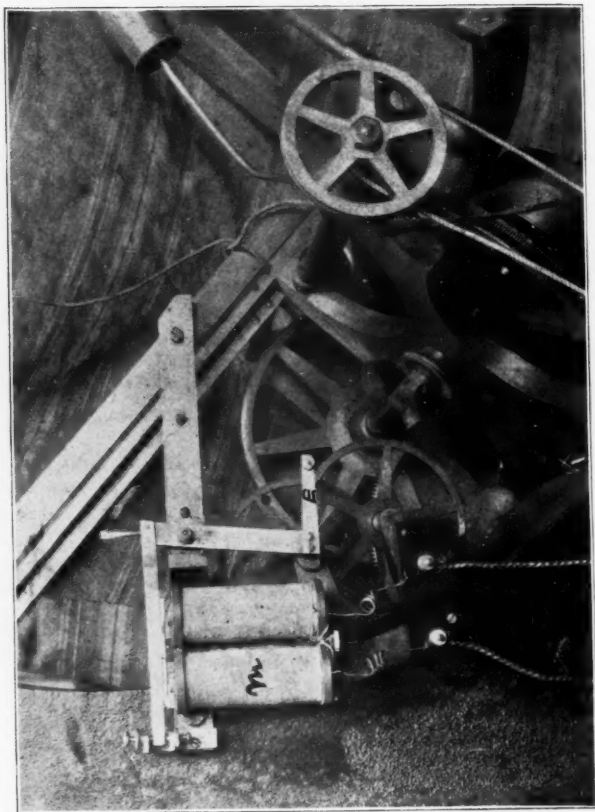
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PLATE XVII.



ELECTRICAL CONTROL OF EQUATORIAL.

G. W. HOUGH, DEARBORN OBSERVATORY, NORTHWESTERN UNIVERSITY, EVANSTON, ILL.

ASTRONOMY AND ASTRO-PHYSICS, AUGUST, 1894.

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General Astronomy.

PHOTOGRAPHIC DETERMINATION OF STELLAR MOTIONS.*

EDWARD C. PICKERING.

A great advantage of photography as a means of studying astronomical phenomena is the ease with which a vast number of facts may be collected. These facts are recorded in permanent form and later may be deduced or verified. An attempt has accordingly been made at the Harvard College Observatory to detect by photography stars undergoing considerable change in position either owing to parallax or proper motion. Two methods have been employed for many years in comparing photographs of the same parts of the sky. First, by superposing two negatives of the same region, in which case the images of the stars will form curious sets of concentric circles, if the plates are not exactly oriented. Since the two films are separated by the thickness of one plate of glass, small differences in position cannot readily be detected in this way. Secondly, if a contact print is made from one negative and the other negative is superposed on the positive thus formed, the dark images of one plate should fill the light spaces in the other, and give a nearly uniform surface. In actual practice this method was found here less satisfactory than might be anticipated, although highly recommended by Professor Barnard (*Astron. Nach.* CXXX, 77). A cluster appears to be the best object for this method. If the plates are not exactly superposed each star appears to project slightly from the background and to cast a shadow on one side.

A third method has accordingly been tried. A photographic plate is placed in the plate-holder with the film side away from the objective, the photograph being taken through the glass. The character of the images thus obtained does not seem to be affected. Theoretically, the plate-holder should be moved towards the objective by two-thirds the thickness of the plate, but this correction is scarcely perceptible, with a large telescope. When such a photograph is superposed upon a photograph taken in the us-

* Communicated by the Author.

ual way, the two films being placed together, all the images of one should appear to coincide with those of the other the difference in the planes of the two images not being noticeable even when viewed under a considerable magnifying power.

This last method has been in use for the past three years at the Harvard College Observatory with the 11-inch Draper telescope at Cambridge and with the 13-inch Boyden telescope at Arequipa. A number of objects, about a hundred and fifty in all, have been selected, including variable stars of long period, of short period, Algol stars, stars whose spectrum is of the third, fourth or fifth type, binary stars, stars having large proper motion, etc. The two dates are computed on which the longitude of the Sun should differ 90° from the longitude of each of these objects. Within a few days of each of these dates, two photographs are taken one with the plate in the usual position, the other with its film reversed. Generally, an exposure of about ten minutes was given to each. The first of these plates was taken in August, 1891.

The examination is made by superposing one plate upon another taken six months later and in the opposite position. Thus a plate taken in January with the film turned toward the objective is placed upon the plate taken in July with the film turned from the objective. The other two plates are also superposed. Instead of making the images exactly coincide, one plate is moved so that all of its images shall be exactly north of those on the other plate by a very small amount, for example, $10''$. The plates then appear to be covered with double stars having the same position angles and distances with components nearly equal in brightness. If now the two images of any star differ in brightness it may be variable, and if the position angle is different from that of the adjacent stars it may be suspected of proper motion, or of sensible parallax. In any case, confirmation may be obtained at once from the other pair of plates. A third pair of plates should always be taken one or more years after one of the first dates and thus serve to distinguish between parallax and proper motion. In the actual examination a microscope is used having a field rather more than a centimetre in diameter and traversed by a vertical cross-wire. Sweeps are made moving the plate in right ascension, and after each sweep changing the declination by moving the plate one centimetre at a time, until the whole plate has been examined. As each star in turn is brought past the cross-wire the direction of the line connecting its components is determined with much accuracy. The diameter of the images should not exceed two to four seconds of arc and they

may be placed about ten seconds apart. A parallax of half a second will then change the position angle by about five degrees, a very noticeable quantity. The conditions under which the plates are taken give nearly the maximum value of the displacement in right ascension due to parallax. This corresponds to a change in position angle to which the eye is much more sensitive than to changes in distance. Any suspected objects are marked upon the photographs and confirmed or not on the other plates. If the change in position is real, the same method may be used with great advantage for determining its amount. In the usual method, it is necessary to measure the position of each star from several adjacent companions which involves measurements of several hundred seconds and the application of various corrections for difference of scale, orientation, differential refraction, etc. The results are then brought together and the outstanding differences are discussed. In the present case the work is purely differential. We have only to measure the position angles and distances of what appear to be double stars whose components are nearly equal and to which any convenient position angle and distance may be given. Personal equation dependent on position angle is evidently eliminated. Such effects as differences in scale of the two plates, error in orientation and differential refraction then appear only as linear terms whose values are readily determined. Incidentally an inspection of these plates shows that no slipping of the film is sensible, but such a source of error, if it occurred, would affect all determinations of position from photographs. Although considerable progress has been made in taking the photographs much time has not yet been spent in studying them. An examination has been made by Miss L. D. Wells of 1436 stars on eight pairs of plates, and shows that probably none of these stars have a parallax of as much as half a second. Ordinarily, a pair of plates can be examined in about half an hour.

A preliminary measure of the positions of the images of eight stars in the vicinity of the variable star *T Cassiopeiae*, gave the average deviation of the uncorrected differences $0''.23$ which would correspond to a probable error in the parallax of but little over a tenth of a second as derived from measures of a single pair of plates.

Evidently in a few years the value of these plates will be greatly increased as a means of measuring proper motions. In ten years the work should be repeated and a proper motion of one-tenth of a second would then give a displacement of a second, which, as shown above, would be readily detected by inspection and could be measured with accuracy.

The question has presented itself how far it may be best to photograph the entire sky with the Bruce telescope with plates in both positions, for determining the proper motion of large numbers of stars. The advantages of this method increase in many respects with the focal length of the telescope. The ease with which photographs suitable for this work may be taken with any photographic telescope is the reason for the present publication of a description of this method.

HARVARD COLLEGE OBSERVATORY, Cambridge, U. S.

JULY 8, 1894.

AN ELECTRICAL CONTROL FOR THE EQUATORIAL.*

G. W. HOUGH.

The increased application of photography to astronomical work, makes it desirable to secure uniform motion for the telescope with which the photographic plate is connected. It is not only necessary, however, to secure uniform motion, but this motion must be precisely the same as the apparent diurnal motion of the heavens.

The use of electricity for controlling the equatorial is not new; during the past few years a number of instruments have been provided with electrical attachments of various kinds. In the application of the electro-magnet, to the telescope, however, no new principles are involved which were not worked out many years ago in connection with the chronograph.

More than ten years ago, Professor S. W. Burnham and myself discussed the feasibility of an electrical control for the 18½-inch refractor of the Dearborn Observatory; but so long as the driving-clock ran fairly well, I did not appreciate the importance of anything better, and hence nothing was done in the matter.

The driving-clock of the 18½-inch is a Bond spring-governor—this apparatus by means of occasional repairs and adjustments ran perhaps as well as a majority of such appliances. The rate, however, changed from night to night, and in cold weather the change of rate was so considerable as to become a source of great annoyance in making micrometer measures.

An electrical control for securing uniform circular motion, was first applied in 1849, to a disk chronograph, by Professor O. M. Mitchel, Director of the Cincinnati Observatory. Professor Mitch-

* Communicated by the author.

el applied the electro-magnet directly to the driving shaft of the chronograph and hence it was required to be of sufficient power to sustain the whole of the driving weight if necessary. During my use of this chronograph, it occurred to me that much less work would be required to be done by the electro-magnet, provided the controlling power was applied to a rapidly revolving shaft.

In 1869, I constructed a recording chronograph, in which the electrical control was applied to a shaft revolving once each second. I subsequently used the same method for my recording and printing chronographs.

A short time since I concluded to apply the same control to the 18½-inch equatorial of the Dearborn Observatory. As this method of control is exceedingly simple and is also positive in its action, a description of its salient features may be of value.

The pendulum and escapement arms of the driving-clock were removed and an electro-magnet substituted as shown in the diagram. The office of the arm, E, of the electro-magnet, M, is simply to hold the clock-work until unlocked by the operation of the electro-magnet. The clock-train, in this apparatus is controlled by a fan, and is regulated to always run fast. The rate of the telescope driving train, when controlled by the electro-magnet, must therefore be precisely the same as the rate of the clock which operates the electro-magnet, M. In other words the control is absolute; the only error being in the imperfection of the worm-gear. The shaft which is locked by the electro-magnet makes a revolution in about 1.016 seconds and hence the standard clock for operating the magnet must be rated to lose approximately 60 seconds hourly on sidereal time. The clock used in connection with the equatorial, has a compensated seconds' pendulum and Graham escapement. As this control has been in use only a short time, the error due to the worm has not been ascertained with great precision; it will, however, approximate 10" of arc.

For micrometer work a slight irregularity in the motion is a matter of no consequence, but for photography, the worm should be as perfect as possible.

The following is an average specimen of the performance of the driving-clock controlled electrically.

The first column is the sidereal time, the second, the displacement of a star as measured with the micrometer.

April 26.	Sid. T.	Error	Sid. T.	Error.
	10:30	— 0.0	10:55	— 9.0
	35	— 5.0	11:00	— 6.2
	40	— 8.7	05	— 6.2
	45	— 8.0	10	— 6.5
	50	— 8.0	15	— 2.5

From this table it is seen that the maximum due to the worm displacement of 45 minutes was 9" of arc. The displacement would be no greater than this for any interval of time.

I have made stellar photographs of the cluster in Cancer, with the finder 47-inch focus, with an exposure of 20 or 30 minutes, without adjustment during the exposure.

The star disks appear practically round when examined under a microscope.

For short-focus star-cameras, the displacement due to imperfections in the worm would be a matter of no moment, and it seems feasible to make such photographs automatically, and without the constant attention of the astronomer.

There are in the United States a number of Clark telescopes provided with the Bond spring-governor, which may be electrically controlled at a very trifling expense. No change is required in the clock-work, but simply to lock the revolving arm on one side only, by means of an electro-magnet, in place of the escapement arms which are removed.

The advantage of the absolute control for ordinary micrometer work is so great, that any one who has used this method would not be satisfied with a spring-governor, or any other form of unstable control.

The spring-governor, as is well known, locks the train twice every second, and theoretically is better than a seconds' control, but practically, both for the chronograph and the equatorial, it is not necessary. The disturbance of the telescope, even with the highest magnifying power, is scarcely perceptible, when the locking is performed only once each second. By using a double locking arm, however, and a half-second pendulum for operating the electro-magnet, this objection is removed.

For a long focus telescope like the 36-inch Lick or the 40-inch Yerkes of Chicago, theoretically a half second control would be preferable. Owing, however, to the vibrations of a long tube by the wind, one cannot secure very great stability with the ordinary equatorial mounting, and hence the disturbance due to the control is hardly worth consideration.

In the January and April numbers of this journal, Professor W. H. Pickering and F. L. O. Wadsworth have proposed the use of

an electric-motor as the prime mover to carry an equatorial telescope. The use of an electric-motor for winding the clock and for shifting the telescope or other apparatus, in many cases is of great value and is to be commended; but its use as a prime mover for securing uniform motion at a fixed rate of speed, in my opinion is not a promising problem. The speed of the motor will depend on the electro-motive force of the circuit which is not constant.

An electric-motor has already been used as the prime mover for a chronograph.

About forty years ago it was thought that a true gravity pendulum could be secured by employing an electro-magnet to unlock the gravity arm. In neither of the cases above mentioned was the result satisfactory, for the reason that the strength of the electric current is not the same for any considerable period.

If a motor is used to drive the telescope, nothing would be saved in the way of clock gearing, and I imagine the control for rate would be more difficult than if gravity from the fall of a weight were employed.

In the practical use of an equatorial, the great desideratum is a driving-clock which will start instantly and carry the telescope at a uniform and fixed rate. In my experience, I have lost a good deal of time and occasionally important observations from the bad performance of the driving-clock. If a driving-clock is used which will take care of itself, then one's whole attention can be given to observation.

DEARBORN OBSERVATORY, Northwestern University,
May 14th, 1894.

SOME NEW FORMS OF DOUBLE MOTION MECHANISM.*

F. L. O. WADSWORTH.

I have used the term Double Motion Mechanism to designate that form of mechanical movement used in spectroscope slits, astronomical refractometers, and similar pieces of apparatus, for moving two jaws or carriages simultaneously in opposite directions, at the same speed. The usual mechanism employed for this purpose consists either of a right and left hand screw longitudinally fixed and working in nuts on the movable carriages, or a system of link work symmetrical about the central line from

* Communicated by the author.

which the motion takes place. Each form of mechanism has disadvantages peculiar to itself, and in order to bring out more clearly the nature of the improvements which it is the object of the present paper to describe, the principal points which will govern the choice of a mechanism for this particular purpose will be briefly considered. They are:

1. Accuracy of movement.
2. Ease and rapidity of movement.
3. Compactness of mechanism for a given range of movement.
4. Simplicity and cheapness.

As regards the first of these points, the screw system is mechanically superior to any form of link mechanism, because of the number of points at which errors due to imperfect fitting are possible in the latter. Indeed it is now possible by Rowland's method to make a single screw accurate, so far as uniformity of pitch is concerned, to almost any required degree. The slight difference in pitch between the right and left hand portions of the screw may, if great accuracy is required, be compensated by making the driving nuts independent and separate from the driven jaws or carriages (as in a dividing engine), and suitably inclining or curving the guides along which the tail pieces of these nuts slide. The main cause which tends to inaccuracy in the use of the screw is the necessity for placing it, in most cases, unsymmetrically with respect to the guides for the carriage, in order to avoid obstructing the central part of the field. Then the point of application of the driving force is much nearer one side of the carriages than the other, and there is in consequence a tendency to twist the latter in their ways, which can only be prevented by making the bearing surface very long, or by using spring gibs, weights, or equivalent mechanical means for bringing the center of resistance to motion into the line of application of the driving force. The first and better method increases the necessary size and bulk of the instrument, and the second is unmechanical, while both considerably increase the friction.

As regards ease of motion there is no question of the superiority of a well designed and a well constructed system of link work, for not only may the driving force be more directly and centrally applied, but the links which move the carriages may also be used to support and guide them, and all sliding friction thereby avoided. The motion too is more perfectly under control than with the screw, and may be made much more rapid without increasing the tendency to vibration or jar. Unfortunately, if any

considerable range of motion has to be provided for, the necessary link mechanism becomes bulky and cumbersome, and if compactness is essential the screw is almost a necessity. The last point is however of minor importance except in the case of large instruments when it needs to be carefully considered.

The fourth point is worthy of more consideration than it ordinarily receives. Upon the simplicity of the mechanical design depends, in the first place, its successful operation under the severe conditions of actual usage. It may be taken almost as a general principle that a change of design or construction which cheapens first cost without impairing efficiency, makes the instrument simpler and to that extent more reliable. To explain more clearly what is meant by diminishing cost without impairing efficiency, it may be stated that work which can be done on the lathe is in general cheaper and at the same time more accurate than work finished on the planer or milling machine, and that parts so shaped that they may be made up from standard stock material cost much less than they would if they required special castings to be made for them. Unfortunately the scientist who designs the instrument is not usually acquainted with the details of the mechanical work and is therefore unable to determine the best and cheapest construction for the whole or for any given part, while the mechanic on the other hand, while perfectly prepared to answer the latter question when he fully understands the purpose to be achieved, is oftentimes liable to make grave blunders if left to work out details by himself.

As to relative cheapness and simplicity of these two forms of mechanism much depends on the particular instrument in which it is to be used. In general the link form will be cheapest and simplest in those instruments not requiring great accuracy, nor involving a large range of movement, and the screw form in instruments where these latter qualities are of primary importance.

From what has preceded it is evident that the point of greatest advantage possessed by the link form of mechanism is the ease and smoothness of motion, and that its greatest point of disadvantage is its bulkiness and want of accuracy. The problem with it is to combine compactness with a large range of motion, and accuracy with ease of motion and simplicity. In the case of the screw the main problem is to secure a central driving action on the carriages. To show how these objects have been, to some degree, at any rate, accomplished in particular cases, I will briefly describe some instruments representative of those mentioned at the beginning of the article.

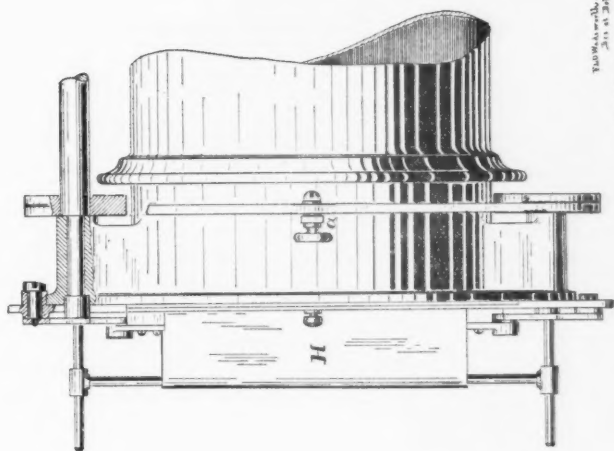
The first one is that form of astronomical refractometer recently invented by Michelson* which consists of two moveable slits placed in front of the objective of an ordinary telescope. In the first instrument of this kind constructed† the slit plates were moved in an out by means of a right and left hand screw geared to a rod running along the side of the telescope to the observer. Twisting of the slit plates by reason of the unsymmetrical position of the screw was in this case prevented by the use of spring gibs which held the jaws in close contact with the guide nearest the screw. This instrument was, however, unsatisfactory in its operation because of the slowness and stiffness of the motion. To overcome these difficulties in the second instrument a form of link work similar to that used in many spectroscope slits was tried. This instrument is shown in Plate I. The two slit jaws, A, B, are pivoted to and moved by the double-ended cranks, C, D, which are themselves journaled on the short tube which slides over the end of the telescope. If either one of these cranks are turned the other will of course turn with it, the two slit plates acting as connecting rods, while they themselves receive a lateral motion. In order to make the arrangement as compact as possible the parts are so arranged that the cranks have a motion through nearly 180 degrees, enabling their full throw to be utilized. To carry the driver-crank past its dead center it was necessary to gear the two together by means of two steel bands (shown by the dotted lines), which were carried around the cap by means of two adjustable idle pulleys, *a*, *b*, and fastened to two drums on the crank shafts. A folding screen, HH, whose function was to keep that part of the objective between the slit plates covered, completes the arrangement, whose methods of operation will be readily understood from the drawings and the description just given. Although far better as regards ease of motion etc., than the first instrument, it was not entirely satisfactory. Unless the steel bands were quite tight, which caused considerable friction, there was more or less binding and sticking as the cranks passed through the dead center position. Again there was nothing to support the slit plates at the outer sides, except the face of the mounting and there was in consequence considerable friction between them and this face.

In the third and still larger 12-inch instrument with which the diameters of Jupiter's satellites were measured by Prof. Michel-

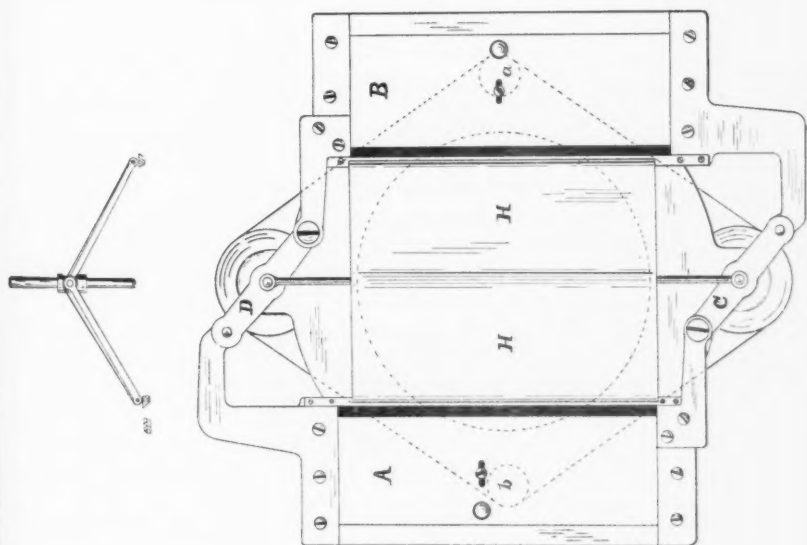
*Application of Interference Methods to astronomical instruments. A. A. Michelson, *Phil. Mag.* July, 1890

† *Ibid.* Fig. I, Plate II.

PLATE I.



Труды Инженерно-механического
Института
С.-Петербургского
Университета
— 1904 г. —



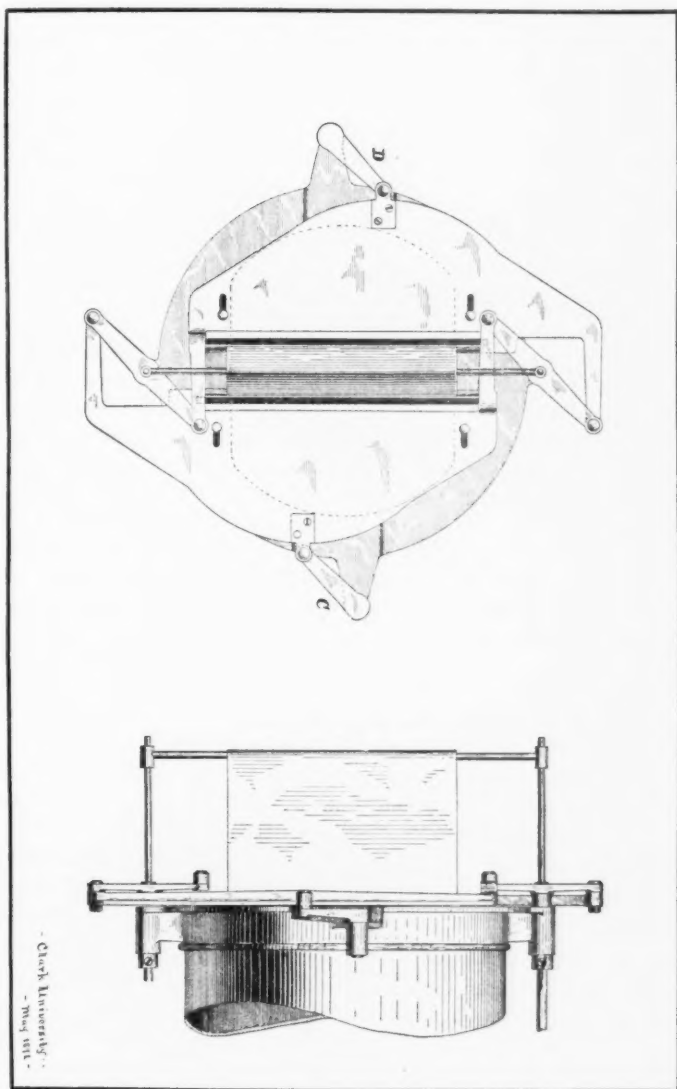
son* at Lick Observatory, these last difficulties were completely overcome by mounting each slit plate on three cranks instead of two as in the previous design. A rough sketch of the instrument is given in the paper referred to below but for the sake of comparison and discussion the more detailed original drawings are here reproduced (Plate II).

In general, as will be seen by an inspection of the drawing, the design is the same as in the instrument just described. The novelty and great improvement lies in the introduction of two additional cranks or links, D, C, placed about 90 degrees from the two double-ended driving cranks, A, B. Each slit plate therefore swings on three points of support, entirely eliminating any sliding friction between the plate and the face of the cap. But the main point of advantage which this arrangement possesses is that with it there is no longer a point of dead centers, and hence not only is the smoothness and ease of motion improved, but the accuracy is also increased, as the slit jaws must always remain rigidly parallel to each other and at equal distance from the line joining the centers of rotation of the two double-ended cranks. The only mechanical conditions which it is necessary to fulfil, are that the six crank elements shall be of equal length; that the two elements of the double-ended cranks, A, B, shall lie in the same straight line, and that the axes of rotation shall all be parallel; all of which conditions may be fulfilled to a degree of accuracy at least as great as would be reached, with the same degree of labor, with any form of screw movement. Accuracy, however, is not so important in this case as those qualities which have already been pointed out as characteristic of link mechanism, together with that one not usually possessed by it, viz., compactness. How far this latter feature has been secured in the present design may be judged from the fact that when closed no part of the mechanism, which has a range of motion equal to the full aperture of the telescope, in this case 12 inches, projects more than $3\frac{1}{4}$ inches beyond the side of the telescope tube at the objective end; an amount barely sufficient to allow the driving rod, which passes back to the observer at the eye-piece end, to clear the enlarged central portion of the telescope tube.

It is evident that the use of three-crank mechanism, if such it may be termed, is of wide application. The high efficiency of link-work trains, together with their cheapness and durability, render the use of them extremely desirable in mechanism, and


* Measurement of Jupiter's Satellites by Interference. A. A. Michelson. Publications of the Astronomical Society of the Pacific, 1891, Vol. III. p. 274.

PLATE II.



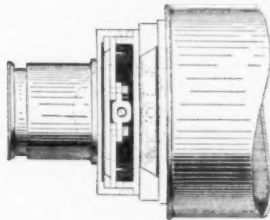
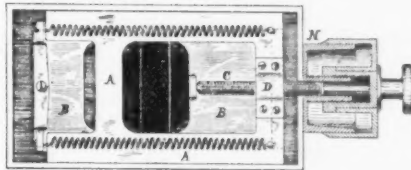
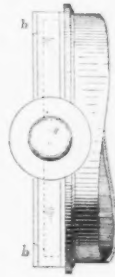
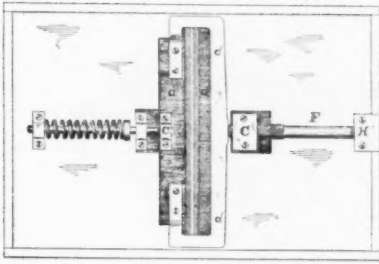
they would no doubt be more universally used were it not for the fact that special means are usually required to overcome the dead points. In the case of the two shafts which are to be driven, one from the other, at a constant velocity ratio (the driving shaft of a locomotive for example), this is done by placing on each shaft two cranks set at an angle to each other (usually a right angle), and using two connecting rods. The three-crank mechanism accomplishes the desired result in a considerably simpler, more compact and less expensive manner, since it requires only one set of cranks and one connecting rod. Simple and efficient as this device is I have never seen it described in any book of mechanical movements nor in any previous publication; and if it has been before used it does not at any rate appear to have been commonly known.

The other form of double motion mechanism which I wish to describe is one of recent design and belongs to the screw variety. The object in view was to secure a mechanism which, while retaining all the advantages secured by the use of the screw in the way of accuracy, compactness, and convenience of operation and reading, obviated the single bad feature of dissymmetry already alluded to. The manner in which this was accomplished may best be described by reference to Figs. 1 and 2, and 3 and 4, Plate III, which represent respectively rear and side elevations of a large double-motion slit and double-motion micrometer recently constructed after my designs by Grunow; to whose excellence of workmanship the highly successful performance of both of these pieces of apparatus is largely due.

Referring first to the drawings of the slit (which is perhaps incidentally of interest as being the largest spectroscopic slit ever constructed, it having a clear opening of ten cm.)* Fig. 1 is a rear view, the back plate being removed to show the mechanism clearly. The two-slit jaws, *a, a*, slide between guides, *b, b*, screwed to the front of the slit plate, and each has a lug, *C*, extending through this plate and projecting about 1 cm., behind it. To the left hand jaw is screwed a  shaped bar, *D*, the side arms of which pass around the ends of the slit and the center of which is opposite the lug on the right hand jaw. The lug, *C*, is tapped to receive a screw, *F*, on which are two threaded portions, one engaging with the thread in the lug and the other of just one-half the pitch of the first, engaging in a nut, *H*, which is screwed to the slit plate.

* This giant slit is used with the great salt train recently completed by Brashear (described in this journal for April) in the bolometric investigation of the infra-red solar spectrum now being carried on at the Astro-Physical Observatory.

PLATE III.



FLOW

When therefore the screw is turned, say to the right, the jaws are separated by an amount equal to the motion of the point of the screw with reference to the lug C, say a distance x , while the whole jaw system is drawn to the right by the action of the fixed nut H, a distance equal to $\frac{1}{2}x$. The center of the slit, therefore, remains fixed, the jaws opening out from it. A spring bearing against the lug on the left hand jaw provides for the return motion and takes up all back lash in the screw. A circular graduated head, M, gives by its motion over a longitudinally graduated drum, N, the whole number of turns and fractions of a turn enabling the exact width of the slit to be determined at a glance.

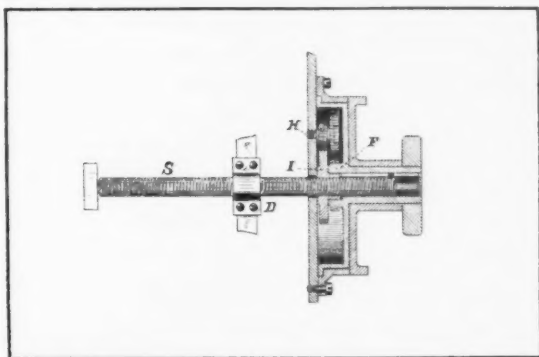
Aside from achieving the chief result aimed at, viz., a central driving action on both jaws, which avoids any tendency to twist and cramp the latter in their guides, and therefore permits shorter slit plates to be successfully used, this form of mechanism presents other advantages over the usual right and left hand screw form. It will be noted that the accuracy of separation depends only on the accuracy of the screw which works in C, the screw at N serving only to keep the jaws centered. In determining the distance between the two jaws we have to deal with the errors of only one screw instead of two as in the case of the right and left hand screw movement.* Still another advantage over the latter is that the motion is positive in one direction only (on opening) and there is therefore no danger of jamming the edges of the slit by turning the screw backward too far.

The drawings of the double-motion micrometer, Figs. 3 and 4, Plate III, are given to show another application of this mechanism and also to illustrate a very convenient form of micrometer for certain kinds of measurement in which it is desirable to have the two images, whose distance apart is to be determined, symmetrically situated in the field of the observing eye-piece. The general design is the same as in the case of the spectroscope slit. One set of micrometer wires is carried on the outer carriage, A, A, which slides between the sides of the micrometer case as in the usual construction. The other set is attached to a second carriage, B, B, which slides between guides on the first one. The separation of the two carriages is effected by the screw, C, working in a nut, D, on the outer carriage, and bearing against a lug on the inner one, the return motion being effected by the spiral springs. The centering of the system is effected as before by a second screw of one-half the pitch of C, which, for the purpose of

* This is of less importance in the case of a spectroscope slit than in the case of the double-motion micrometers next to be described.

securing compactness, is cut on the hub of the graduated drum, N. This method of construction makes the micrometer very compact, while securing great length for the slides. The whole is to be mounted as shown on a second slide on the end of the telescope simply for convenience in placing it at any part of the field of the latter.

One modification of the last form of mechanism which I have only recently designed is interesting, if only in showing how the required motion may be obtained by means of a single screw of the same pitch throughout. This may be accomplished by the same arrangements of parts as before except that the centering nut, instead of being fixed, revolves at one-half the speed of rotation of the screw. In the accompanying figure which shows one simple means for accomplishing it, S is the screw working in the nut, D, on the one carriage and bearing against the lug on the other, as before. In this case the graduated head is held longitudinally and drives the screw by means of a pin or key working in a deep groove cut in the hub of the head. Attached to the head is a fine tooth gear, F, which by means of the two idle gears carried on a fixed pin, H, drive the toothed nut, K, at just one



half the speed at which the screw itself revolves. The drawing itself shows details of construction. The particular advantages of this special arrangement are greater simplicity in making the screw and greater compactness when a considerable range of motion is necessary. If the two carriages to which the desired motion is to be imparted are at a considerable distance apart, as in the second form of astronomical interference instrument described in the paper to which reference has already been made, it

would be better to place the driving wheel and centering nut between the lug on one carriage and the nut on the other. The centering nut, it will be observed, obviates the necessity for any end-thrust bearings and thus eliminates another source of error in the determination of the position of the two carriages.

These two forms of mechanism; the link form, Plate II, and the screw form, Plate III, with these various modifications, which I have described, are only a few of many which I have designed for different purposes, but I have preferred to describe only those which have stood the test of actual usage and have been found more satisfactory than those forms previously in use.

ASTRO-PHYSICAL OBSERVATORY,
Washington, D. C., April, 1894.

MARS.*

PERCIVAL LOWELL.

On May 31st, the date at which these observations of mine began, Mars was about 98 millions of miles distant from the Earth, his south pole was tilted $23\frac{1}{2}$ degrees toward her, and he showed gibbous to the extent of nearly one-sixth of his disk. The phase axis lay slightly to the left of the polar one.

These conditions continued substantially unchanged throughout the period covered by the observations, that is, from May 31st to June 24th; the phase reaching its maximum of 47 degrees on June 16th and the tilt of the pole its greatest latitude of 24 degrees on June 22d (Marth). The phase axis meanwhile slowly shifted its position from the left to the right of the polar one. The planet passed through quadrature on June 16th.

The observations were thus made from four and a half to four months before opposition and about equally long before the time of the planet's nearest approach; inasmuch as opposition will occur on the 20th of next October, and the minimum distance between the two planets be reached on the 13th of the same month. For satisfactory views to be had so unprecedentedly long before, so to speak, the event, the seeing, to secure which as good as possible the Observatory site was chosen, is responsible. What that seeing was the accompanying drawings will show. It was such as to enable me to make out a dozen of Schiaparelli's canals *two months and a half before the summer solstice of Mars' southern hemisphere*.

* Communicated by the author.

Most of the observations and all of the drawings, except the first, were made with the 18-inch. I may say here, however, that in questions of planetary detail of the inner planets of the solar system, up to and including Mars, size of instrument is quite secondary to quality of atmosphere. Large objectives give more light than small ones; that is their chief advantage. For faint stars this light is invaluable, but for some of the planets such illumination were better away. Venus and Mercury are best studied in the daytime for this reason. Now want of light is not the difficulty with Mars. During the dark the planet's glare was too great in the 18-inch. Details invariably came out best about three-quarters of an hour after sunrise. Yet the seeing at that hour was on the whole less good than earlier. From these observations and from certain ones of M. Flammarion, I am convinced that the post-sunrise and pre-sunset hours are the best ones for studying the planet. At other times a very faint dark glass might prove advantageous, or better yet, diaphragming the instrument down; then its focal length will tell over a smaller one. Simply increasing the power will not do, for atmosphere, owing to its inevitable lack of homogeneity, prescribes a limit. With the 18-inch at Flagstaff, a power of 420 was about this limit on Mars, although I have used 1280 without sensible loss, though also without sensible gain. The greater number of my drawings were made with 370. On the six-inch 270 showed well, lake Tithonus and the Agathodæmon canal being very evident as a dark line, and Argyre and Pyrrhae coming out with singular prominence—the dark and light markings being more contrasted, it is worth mentioning, than in the 18-inch.

How little this matter of atmosphere is duly appreciated is evidenced by the way in which a large instrument is assumed to be necessarily superior to a small one, quite irrespective of what it is that is to be observed. Now the fact is that there are two quite different classes of celestial phenomena; those dependent on quantity of light and those dependent on quality of definition for their visibility, and the two means to these ends go anything but hand in hand. For the one, the illumination, the size of the instrument is the prime requisite; for the other, the definition, the atmosphere is the first essential. As an object-lesson in this it is worth noticing that the biggest instruments have not always given the best views of Mars. In matters of Martian detail it is amply evident from the results that observer, atmosphere, instrument is the order of weight to be given as the factors of an observation.

One preliminary result of the Arizona air may be worth noticing in the interest of astronomical concord, always so desirable. The quality of the seeing there suggests reconciliation between the very various drawings made generally of the planet. For Arizona seeing, too, had its ups and downs. When the seeing was exceptionally bad, Mars looked strikingly like his own photographs, as I remarked encouragingly to his photographer. A fact worthy of a word, for it seems to be overlooked in these days when celestial photography is the fashion—overlooked, not by the photographers but by the public, as I judge by more than one question put to me on the subject—that though photography will reveal what no eye can see, the eye will reveal what no photographic plate can show. Planetary detail falls under the latter head. For the difficulty of photographing such detail is not simply, as the inquirers suppose, question of a driving clock timed first to the Earth's rotation and then to the planet's pace, which alone would require more perfect apparatus and a more complicated one than any yet devised. The deep difficulty lies in our own atmosphere, which is never steady enough; what is disclosed one minute being swamped by light waves the next. The attentive eye registers each glimpse, the photographic plate only the aggregate, and in the composite picture thus obtained the bad obliterates the good. With faint stars there is no such loss and every gain. For light is all that is wanted, however it be got. Now the eye cannot add impressions; the camera can. If therefore the camera be exposed long enough it will reproduce what the eye could never detect. But, in the case of detail, the longer the plate is exposed the more certainly will the detail be lost. Until, therefore, we rise above our atmosphere or find an absolutely faultless spot, we shall never be able for planetary work, to match the eye by any film, however sensitive or accurately driven.

In the next higher stage toward atmospheric steadiness Mars assumed his old-fashioned, orthodox appearance, quite guiltless of canals and such-like inexplicabilities, an appearance which the admirable drawings of Mr. Green have raised to the rank of portraiture, and to the finality of which the Royal Astronomical Society, by its skepticism of Schiaparelli, has done so much to commit itself. Under better air, however, the disc began to show symptoms of heresy to these established Anglican views; until finally in the best moments the planet's surface came out with all the clear-cut character of a steel engraving, as Schiaparelli and Terby have described it. From the manner in which these

MARS.



FIG. 1.—May 31, 17^h 48^m (Standard Mountain Time.) Longitude* of Center of Disk 310°; Power, 240. The Hour-glass Sea on the extreme left. Strait visible connecting it with the Polar Sea. Aëria much the brightest land on the disk. Deucalionis and Noachis very faint.



FIG. 2.—June 2, 16^h 50^m (S. M. T.) Longitude 276°; Power, 420. Lybia visible as a sort of snout. Having less sea next to the darkest marking on the disk. The Polar Sea the darkest.



FIG. 3.—June 3, 17^h 40^m. (S. M. T.) Longitude 278°; Power, 320. The beginning of the Nilosyrts visible as a pig tail to the Hour-glass Sea. Strait between Polar Sea and Hour-glass Sea well seen.

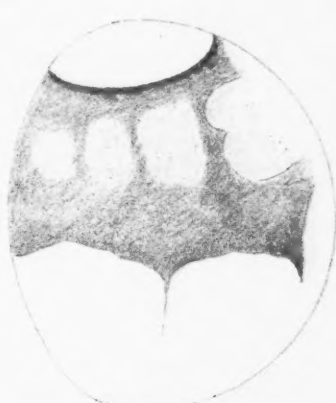


FIG. 4.—June 7, 17^h 45^m—18^h 15^m. (S. M. T.) Long. 244°; Power, 370. Star points on following end of snow-cap. Preceding part dusky. Aëria on the limb, brightest part of the disk except the polar cap. Limb white, utterly unlike land. Beginning of Lethes visible in center of disk, Cloven shape of Hellas shows where Penæas debouches.

* Longitudes are all from Marth. Details seen a few minutes, before and after, included in the drawings.

happy revelations played bo-peep with the eye, it was evident that their intermittent invisibility was not chargeable to Mars' atmosphere but to our own. A canal or a coast line would suddenly stand out perfectly distinct several hundred miles in length, and then as suddenly be waved, by our own air, out of existence. The northern coast of the Mare Sirenum came out in this manner on June 24th, and three canals, centering like the spokes of a cart-wheel about a lake for hub, probably the Araxes, the Eumenides and the Agathodæmon about the Lacus Phœnicis, did the same on June 23d.

So much for the terrestrial conditions under which the observations were made. The Martian ones were such as to make the polar cap and its accompanying phenomena the centre of interest upon the planet.

In the matter of the compass points, I would say that I make use of the orientation proper to the planet, that is east is east to one on the surface of Mars. This is similar to our own terrestrial orientation. The south pole of Mars reached its maximum dip toward the Earth on June 22. It amounted then to 24 degrees, an inclination greater than it has shown us for fifteen years, or will show us for fifteen years to come. Its south polar regions were in consequence peculiarly well displayed, and this at the time when the snows there were in active process of melting.

At the beginning of the period included in these observations, the area covered by the snow-cap was still very large. On June 3 I made it 47 degrees across, that is, it covered nearly the whole frigid zone. During the month it decreased slowly in size. On June 19 it measured about 39 degrees. But I think the former of these determinations somewhat too large and the latter a trifle too small. Throughout all this time its outer edge looked to me to be perfectly elliptical, implying, that is, that it was in fact perfectly circular.

Girdling it was a narrow dark streak of nearly uniform width, as seen at any one observation, but which slowly narrowed as the longitudes grew less. The snow was continuously bordered by this belt, which was broadest between 320° and 220° of longitude. Here calculation showed it to be about 350 miles wide. Farther east, above the Mare Sirenum, it had contracted to the half of this. It was clearly water at the edge of the melting snow, a polar sea in short. And it is worth noting that it was widest where the dark markings, the seas, were greatest in extent. Between Hellas and the Chersonesus its surprising symmetry was broken by an expansion into a great gulf two or three times the

width of the rest of it. Under the best seeing the gulf showed a beautiful deep blue.

On the morning of June 8t, at 1^h 17^m G. M. T., I observed suddenly a couple of extremely brilliant star-like points upon the snow-cap *n. f.* just above this great bay. They were very conspicuous, resembling exactly the starry dazzle from a distant surface, tilted at the proper angle for specular reflection. A few minutes later they had disappeared. Just such an effect would be produced by snow slopes suitably illumined turning through the glinting angle. We have here then, in all probability, evidence of differences of elevation in the snow-cap, of mountains there in short. Their position proved to have been in long. $291^{\circ} 30'$; lat. $75^{\circ} 40'$ south. This, together with the known direction of the Sun at the time, enables us, assuming specular reflection, to tell the tilt of their slopes. At that point upon the disk under the then illumination from the Sun, the tilt of a slope capable of sending specular reflection to the Earth would have been $54\frac{1}{2}^{\circ}$, and the slope must have faced $9\frac{1}{4}^{\circ}$ west of north. This is a surprising steepness, but fortunately absolute specular reflection is not needed to account for the phenomena. At so great a distance a surface tilted at a much lower angle if of high reflective power would produce a like dazzling effect, and for other reasons it is probable that the Martian slopes are not excessive. At the same time phenomena have been observed which seem to require some height in these hills, making them another of the puzzling points about this interesting planet. I shall resume discussion upon the snow-cap in a later paper. Their position, it will be noticed, corresponds with the star-points seen by Green in 1877 and by Mitchell in 1845, only that they were then seen as distant snow islands, the polar cap having disintegrated further.

On the morning of June 10, at 0^h 10^m G. M. T., I saw similar star-points appear in about the same relative position on the disk, less bright than the first ones and from the longitude then on the meridian, situated further to the east of them. I saw such again on the 11th, the 13th and the 14th, at hours which showed them to belong to a long range of heights on that side of the snow-cap. Sometimes they glittered like stars, sometimes they appeared simply as perfectly white spots.

Meanwhile on June 9, I had my first certain view of the great rift. It came out as a dark line roughly parallel to a parallel of latitude in the midst of the snow, about 15° from the lower edge

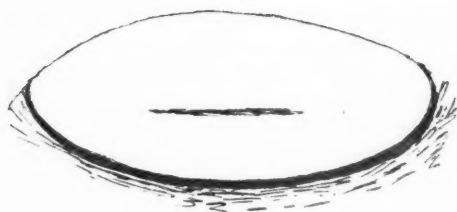


FIG. 5.

the following side of the cap, back of the great polar bay. The phenomena on this morning were particularly detailed. Above the bay the snow glittered with the bright points I have spoken

of, and then back of them fell off shaded to the rift. The rift itself appeared in the form of a half cross or notch in the heart of the snow at the base of the hills.

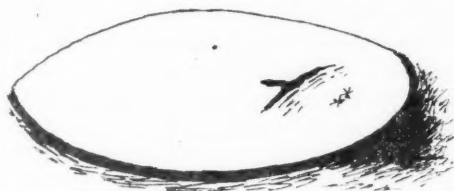


FIG. 6.

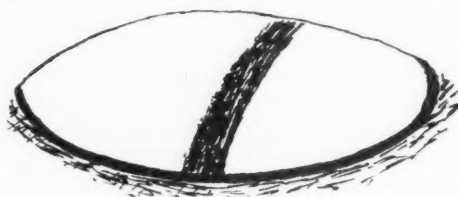


FIG. 7.

end was therefore 160° . The rift was very broad, apparently equally so throughout, and looked like a huge cart-track coming down to one over the snow. On the 15th I estimated its breadth at one-eighth of the cap's diameter. The same morning I also made a comparison of it with the scale of artificial canals. The values computed from the two determinations agreed perfectly, giving it a breadth of 220 miles.

On the 12th I noticed star-points on the preceding side of the cap. This was then to the east of the rift and indicated therefore, high land on that side of it, as well as on the other, tilted also to the west of north. Later the preceding end lost its lustre and the following end had once more become the brighter.

From these observations and others at this Observatory on

of the cap, and nearly half as long as the cap's diameter. After this I saw it under varying angles of rotation, as will appear from the drawings.

On the 13th it showed cornerwise on

On the 14th I marked the rift debouch into the polar sea. At 17^h 30^m, mountain time, that is, 15d 0^h 30^m G. M. T., the entrance to it was on the meridian. The longitude of the rift's eastern



FIG. 8.—MAP OF MARTIAN SOUTH POLE.

The stars represent the brilliant points seen, indicating high land.

the western portion of the rift, I have drawn a plan of the polar-cap and marked it for elevation, as given below. The length of the rift would therefore seem to be about 1200 miles.

Next to the snow-cap the most striking feature about the disc was the emphatic tripartite character of its markings. The surface of the planet was conspicuously divided off into three distinct portions, the continental area, the polar cap and the area of dark markings lying between the two. Though in no sense zones these corresponded in a general way with the equatorial, the south temperate and antarctic regions respectively. Of zones proper, there was, with the exception of the polar cap, no evidence whatever, which means of course that no cloud formations typical of them were visible; Mars being in this respect utterly unlike Jupiter or Saturn, where all that we see is cloud evidence of zone.

The three portions referred to may be recognized in any map of Mars it was their marked contrast that was striking. For next to the sharp manner in which the snow-cap stood out upon the disk was the definite character of the continental coast line and the equally indefinite character of all the markings between the two. The coast line was most salient and clear-cut on the western side of the Hour-glass Sea (Syrtis Major or Mer du Sablier).

To the eastward the coast lay in general direction straight, approaching the pole as it stretched eastward. It was indented by numerous bays but destitute of those comet-tail peninsulas so generally observed connecting it to the chain of islands south. All of these islands Hellas, Ausonia and the rest, were vague, without definite contours and lapsed imperceptibly into the surrounding seas. Even in color they were less decided than, though of much the same tint as, the continental areas.

No connection appeared between the continent and the islands till it came to the eastern end of Mare Sirenum. Here Icaria and Phætontis made a bridge of singularly uniform width, preposterously straight and narrower than usually figured. Beyond this to the east, Thaumasia appeared, at times well defined, at other times not. Lack of contrast not lack of contour was responsible for this, as the glimpses of it showed.

The colors of these three portions of the planet's surface were as marked as their contours. In the best seeing the colors were exquisite. During the dark the polar cap was straw-color, the seas a limpid bluish green, and the lands a brilliant rose-orange. With the sunrise the snow-cap turned to white, the seas to sky-blue and the land to a rosier red. The sky-blue of the seas was almost exactly the same tint, only a trifle fainter, as the blue of the surrounding sky, a sky be it remembered, of over 7000 feet of altitude. The light of the Sun filtered through our atmosphere, added blue to all, thus wiping out the yellow. A similar effect is visible in Venus and the Moon, which from pale yellow by night turn pale white by day.

It may be noted, in passing, that these before and after sunrise hues seem effectually to dispose of the suggestion that the seas owe their color to contrast. Were that so then when the changed light had made the lands less yellow, contrast should make the seas seem more yellow and not, as was the case, more blue.

For purposes of comparison with terrestrial objects, the post-sunrise tints are of course the ones to take. The resemblance in color therefore, between the so-called seas and water scarcely needs comment.

Next in point of general importance come the phenomena of the terminator and limb. The terminator differed alike from the terminator of the Moon and from the terminator of Venus, both of those bodies opportunely offering themselves for comparison in the course of the month. The Martian terminator was more marked than the lunar one, less marked than that of Venus; that is the shading was greater in the one case, less in the other. It

would thus seem to imply for Mars a depth of atmosphere between the two.

The softening of the light was evident the whole length of the lune that was lacking. Even the polar snows showed it, being distinctly shaded on their preceding portion, notably on May 31st and June 7th. The rest of the terminator was similarly darkened, the dark parts being darker and the light less light than the centre of the disk. Only one exception to this did I observe; on the 24th when a light band lay along the edge some twenty-five degrees wide over the sea area. This may have been caused by Argyre and Pyrrhae then upon that portion of the disk though not distinguishably visible.

I saw no irregularities upon the terminator although the terminator of Venus, with the same power, seemed not rigidly correct, either at the cusps or the fringe's inner boundary, and that of the Moon stood conspicuously notched to the naked eye. On the terminator of Mars the only projections were the bright areas which projected in a body, an effect palpably due to irradiation. It seems therefore, safe to say that few if any mountains relatively comparable to the lunar ones, exist on Mars. The elevations otherwise revealed are probably of no great height.

The aspect of the limb was more suggestive, if harder to explain. The limb was very luminous and yellow. This luminosity was not confined to the actual limb, but extended a long distance on to the disk. It was as if a wash of some lighter tint had been laid on around that side of the picture; at times it showed an inner edge of demarkation as abrupt as a wash of water-color. I saw it thus on June 15th. An estimation of its breadth relatively to the radius of the disk made it 29° in from the limb, or two hours in time. As the limb stood at nine o'clock of a Martian morning, this corresponded to 11 o'clock in the day. So abrupt an inner contour is doubtless an effect of contrast but it implies a pretty rapid falling off in the light.

With the exception of the polar cap and its surrounding polar sea, all detail near the limb was lost in this light, and to a distance in from the edge proportionate to the intrinsic contrast of the detail. The surpassing brilliancy of the snow-cap and the darkness of the water beneath it caused these to stand out up to the edge. The sea area below them sometimes showed to the limb, sometimes not; the dark patches nearer the equator, to my eye, never. Roughly speaking, the farther from the pole the point on the surface lay, the farther from the limb was it obliterated.

Reversely every bright portion of the planet was brighter when upon the limb. The most striking instances of this were the islands like Hellas. This when coming around the corner shone with a brilliancy almost rivalling the polar cap. Yet so soon as it was well in evidence it was content to sink almost out of sight during the rest of its journey across the disk. Such first putting in of an appearance and then lapsing into obscurity was a speciality of islands; but once I marked, for some singular reason, both the islands and the coast line—in this instance *Electris* and *Zephyria*—thus singled out for initial prominence. Whether islands and coasts are more covered by moist air is a question suggested by these phenomena.

Such impartiality of obliteration shows, however, one thing: that the light along the limb is not due, as has been suggested for somewhat similar phenomena on the lunar limb, to reflection from mountain slopes or other irregularities of the surface. Nor does it seem attributable to cloud properly, so-called, since we can hardly suppose clouds to be so superior to latitude, or to be so idiosyncratic as to remain till 11 o'clock in the forenoon and no later. We are thus apparently forced to fall back on atmosphere pure and simple, with probably much aqueous vapor in suspension.

Collateral proof of some such homogeneous veil, rendered opaque only by its thickness, appeared on July 15th, when Hellas then on the limb, stood out embossed on the light surface preceding it. The light surface was in fact a dark strait, but the whole had been illuminated by the light of the limb.

We now come to what we have at present every reason to believe to be water, the dark patches. A part of them may be vegetation of which more later; but a part can hardly be anything but water and perhaps all may be.

These dark patches, or, as we may say provisionally, seas were of diverse degrees of darkness but each tint merged imperceptibly into the others. They looked to me to differ in degree, not in kind. The darkest of them was the polar sea with its great polar bay. In the best seeing this last appeared deep blue; its size enabling the color to come out. The next darkest was the Hour-glass Sea (*Syrtis Major*); then the *Syrtis Minor* and the *Sabaeus Sinus*. All these are small patches, irreverently suggesting puddles, or deeper spots in the general water waste. Next to them in tint was the strait between Hellas and *Pyrrhae* connecting the polar and Hour-glass seas.

To the eastward of the *Syrtis Major* the tint of the seas grew

lighter, and the same was true to the west. Indeed the Hour-glass Sea seems to be the centre of the oceanic system of the planet. Not only does it lie at the apex of the largest water area on the planet; it is with the exception of the temporary polar sea, the darkest and therefore presumably the deepest body of water there; but it makes a sort of funnel to discharge these waters into the various canals through the Nilosyrtis, the largest of their number.

This brings us to the canals. The first one I saw was on June 7th, the Cerberus in all probability from its position. On June 9th it appeared persistently. I noted it as looking very broad. When on the meridian I glimpsed it double. I had only one glimpse of it as such and though this was definite enough, I put no very great faith in it. I mention its duplicity for future verification. On the 9th and more certainly on the 10th, another canal showed debouching into the same bay but turning more to the right, as it went northward, the Galaxias perhaps. I next thought I saw in the very middle of the continental area, a part of the Orcus parallel roughly to a parallel of latitude. I never familiarized myself beforehand with the region about to be seen. During the next week I made out Gigas, Titan, Gorgon, Sirenius and Eumenides. The darkest of these was the Eumenides, though it was the least well placed for observation. Up to this time the canals were of great difficulty, and were neither sharp nor phenomenally straight; fugitive glimpses, many of which flitted before me but of which I have recorded only the more certain ones. But when the region of the Lake of the Sun began to come round the canals in its neighborhood came out in much better shape; Phasis, Eumenides and Agathodæmon. Detailed explanation of the drawings will give a better idea than I can do in the text of the observations.

The general outcome of these early observations is suggestive. For the phenomena seem to point to a vast vernal freshet, now in process upon the planet. In the first place we see the great polar snow-cap vanishing under our eyes. Now this might take place either by evaporation or by melting. Doubtless it does take place in both ways, but what is of interest is that we have direct evidence of the latter process. Instead of simply growing beautifully less the snow-cap appeared persistently fringed by a dark line which steadily kept pace with its retreat. This can hardly be anything but water although it may be water in a very different condition from what we know it, neither fresh nor salt, for example, but something unlike either. The character of the wa-

ter is to certain extent a vital question, as irrigation with salt water does not commend itself to our notions.

Now so much water suddenly produced has got to be disposed of. For it is in a state of unstable equilibrium. So long as it was snow it stayed where it was. But the moment it melted gravity would pull it from the pole. Here step in the next observed phenomena. The bluish cast of the south temperate belt and the indistinctness of the contours of the islands there hint that this whole region is undergoing submersion and submersion of a sudden and temporary character. For in the first place we know that at other seasons these islands are both more conspicuous in color and more distinct in form, and that the comet-tail peninsulas now invisible appear with all grades of distinctness. In the second place if this submersion were not of a temporary character the contours of the red portions would be more definite than they are. For a marsh does not remain a marsh for long time; it becomes either land or water, by reason of the forces at work upon it.

Thirdly, the canals of the continent are well known to be less visible at this season of the Martian southern hemisphere. This is precisely what should happen if they are canals, especially if what we see are the effects of the water in them rather than the water itself. For it would take time for the effects of a flood, in the way of vegetation for example, to show themselves. Now it is rather significant in this connection, that the canals now best seen should be those nearest the south pole, namely, the ones about Thaumasia and Mare Sirenum.

So much for the positive evidence of the circulation of water as water upon the planet. The evidence of its circulation as cloud is negative. There is no sign of cloud apparently at this season. Although the air is probably charged with vapor, it does not condense into cloud or fall as rain or snow. Even occasional indistinctness of detail seems, as we have seen earlier in this paper, to be due to our atmosphere, not to that of Mars. At this spring season of the southern hemisphere the Martian aqueous circulation seems therefore to be chiefly a surface one of water.

Here we have a *raison d'être* for the canals. In the absence of spring rains a system of irrigation seems an absolute necessity for Mars if the planet is to support any life upon its great continental areas.

It is specially important that phenomena like these spring phenomena should be reasoned on. For it is chiefly by collation of inference that we are likely to build up a true knowledge of

MARS.

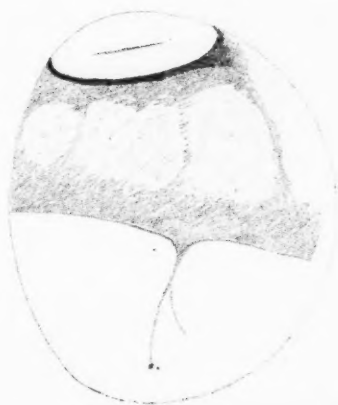


FIG. 9.—June 9. 17^h 20^m (S. M. T.) Long. 214°. Power, 370. Rift in snow-cap. Following part of snow-cap peculiarly brilliant. Cyclops Cerberus (together) and Galaxias in center of disk. The former ends in a black dot, possibly the Trivium Charontis. Great Polar Bay visible on following edge of snow-cap.

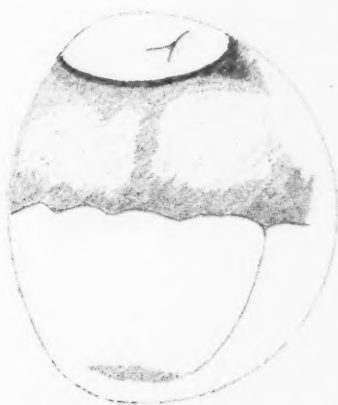


FIG. 10.—June 13. 18^h 40^m (S. M. T.) Long. 194°. Power, 370. Rift well seen. Star points following it. South east of them, the snow-cap lies shaded to the rift. Cyclops and Cerberus on the right of disk. Suspected Trivium Charontis at bottom. Polar Bay well seen.



FIG. 11.—June 15. 17^h 30^m—18^h (S. M. T.) Long. 161°. Power, 640. Rift on the meridian.

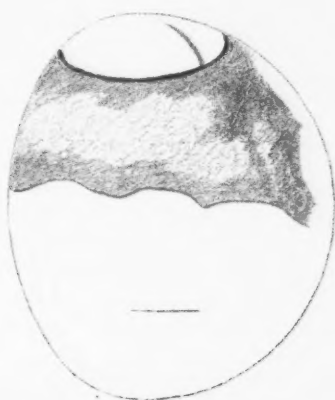


FIG. 12.—June 16. 17^h 20^m (S. M. T.) Long. 145°. Power, 370. Rift on following end of cap. Orcus at the Nodus Gordii in the center towards the bottom of the disk.

MARS.



FIG. 13.—June 17. 18^h 38^m (S. M. T.)
Long. 155°. Power, 370. Titan, Gigas
and Gorgon.

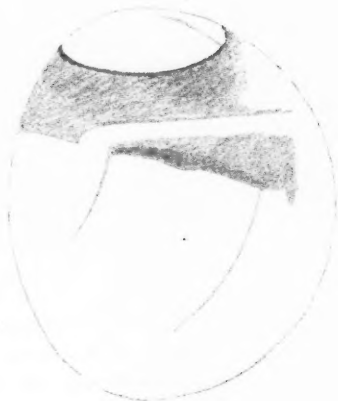


FIG. 14.—June 19. 17^h 45^m (S. M. T.)
Long. 122°. Power, 370. Rift again.
Gigas and Sirenus. Icaria and Phaeton-
tis strangely straight.

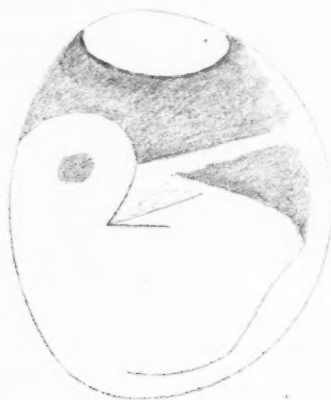


FIG. 15.—June 20. 16^h 40^m (S. M. T.)
Long. 96°. Power, 370. Lake of the
Sun, Phasis, Araxes Eumenides.
Araxes, the boundary of a shaded area.

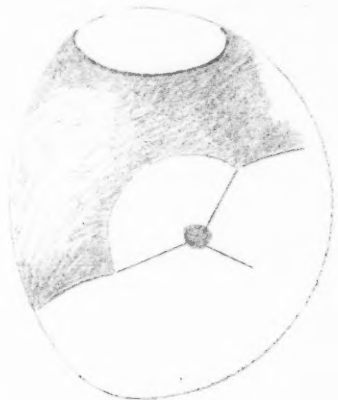


FIG. 16.—June 23. 18^h 20^m (S. M. T.)
Long. 91°. Power, 370. Probably the
Lucus Pœnicis with Agathodæmon,
Araxes and Pyriphlegethon.

this our neighbor world. We shall best explain such enigmas as the canals by getting greater insight into the general physical conditions existing upon the planet. It is by no means inconceivable that we may first succeed in unravelling that net-work of seemingly incomprehensible lines by inevitable inference from comparatively simple data.

LOWELL OBSERVATORY, July, 1894.

THE SEAS OF MARS.*

WILLIAM H PICKERING.

The first observation made upon Mars at the Lowell Observatory with the 18-inch Brashear lens was upon June 1, 1894. Since then observations have been continued upon nearly every night. What appears to me to be the most important conclusion deducible from our work so far is that Mars does not always present the same appearance at the corresponding time upon two successive Arian years. This remark does not apply merely to small details but to large and prominent features. Moreover this difference does not seem to be due simply to the fact that one season is a few weeks later than the other, but that the phenomena presented upon the two years are really different.

Thus the central branch of the Y, just north of *Noachis* which was so marked a phenomenon in 1892, was not visible to me early in June, as I had expected it to be. It is true that Mr. Lowell thought he saw it faintly marked, but although I looked for it upon the same evening, I could not satisfy myself of its existence. Nevertheless the definition was such that had it appeared as it did in 1892, it could not have been missed at the first glance. I looked for it again at the following presentation in July just passed, but no trace of it was to be seen. Two drawings made by Professor Campbell upon July 18 and 20, 1892, and published in the last number of the Publications of the *Astron. Soc. of the Pacific*, p. 171, show it very nicely indeed. These may be compared with some reproductions of my own work now originally published in *ASTRONOMY AND ASTRO-PHYSICS* 1892, p. 668, and now republished in the same number with the drawings of Professor Campbell. After the disappearance of the central branch in the latter part of July, 1892, a portion of it reappeared in August, and remained visible through September. A sketch

* Communicated by the author.

showing its appearance upon September 4, 1892, has been kindly forwarded to me by Mr. Russell of the Sydney Observatory, N. S. W. This branch may therefore be said to have been characteristic of the opposition of 1892. This same region was very carefully sketched by Mr. Douglass and myself a number of times between June 30 and July 6, 1894, but not a trace of the central branch could we detect. Upon these dates Mars held the same position in its orbit that it did upon August 12 and 18, 1892. A sketch made by myself August 13, 1892, shows the central branch very clearly. It will be interesting to hear if its appearance has been noted this year by the Australian observers, since in their longitude it would have been visible about the middle of June.

But not only has the central branch of the Y been invisible this year, but the large dark blue patch which it connected with the southern snow cap, and which we called the Northern Sea, has been very much less marked, and much smaller than was the case in 1892.

Again a large black gulf bounding the melting snow upon the north and situated very nearly due south of *Syrtis Minor* has been a very striking feature of our observations this year. This gulf was only observed once in 1892, upon July 27, and it was then by no means conspicuous. If these very dark regions are, as we suppose them to be, water, it would then seem that the water which did not reach the northern regions this year has appeared as an excess in the south.

Upon testing this black region upon June 4, with an Arago polariscope, made for me by Mr. Brashear, it was found to show clear traces of polarization, as did the canal running north from it. This would naturally be the case if it were water, since being situated near the limb, it would reflect to us largely the light of the Arean atmosphere. Upon the rest of the disc of the planet, the polarization was not very conspicuous. At the next presentation of this region, upon July 9, the observation was repeated but to my surprise no trace of the polarization in the dark spot could be detected. A close examination of the region was then made, and its color was found to have entirely changed,—whereas upon June 9, Mr. Lowell writes "Bay a deep blue, looks just as deep water does," it was now found to be of a rich chocolate brown tint, differing entirely in color from the bluish grey regions to the north of it. These grey regions showed no sign of polarization, and as I have before remarked I see no reason for supposing that their color is due to water. As far as my observations go, it appears to me that the permanent water area upon Mars, if it exists at all, is extremely limited in its dimensions.

These large grey regions were of a brilliant and decided green color in 1890, just before the vernal equinox. In the early part of 1892 also, large green areas were seen upon the planet, but as the season advanced the green regions changed almost entirely to grey. At the present time very little color is visible in the shaded regions. They are subject also to such large variations in area, as the season progresses, that unless we can persuade ourselves that gigantic floods, unaccompanied by clouds, form the normal condition of affairs upon Mars, we seem forced to adopt some other explanation of their existence. The theory that they owe their color to vegetation is perhaps the most plausible one, and some new facts bearing upon this matter have recently come to hand. Upon June 30 a distinct depression in the terminator where it was crossed by the stem of the Y was detected by Mr. Douglass. As the planet rotated, the position of the depression changed, and it was noted that it was not always found in portions of the terminator which was darkest. Since that date similar depressions more or less marked have been detected upon nearly every evening. Upon looking over our observations for 1892, I find under date of September 20, 8^h 06^m a drawing showing a flattened terminator, and a statement that "the planet seems somewhat of this shape." Further investigation shows that the long narrow strip known as *Ceraunius* was lying upon the terminator at about this time. These notches in the terminator can be most readily explained by actual depressions in the surface of the planet, and as Professor Campbell has shown (Pub. Astro. Soc. Pac. 1894, p 110) a difference of elevation of the surface amounting to two miles ought to be readily visible to us on the Earth at certain seasons provided the elevation or depression involved occurred upon the terminator. It thus appears that we are perhaps on the eve of being able to construct a contour map of the planet. The observations involved are however very difficult, and no great accuracy in the results can as yet be expected.

Strictly speaking the notches in the terminator correspond to variations in the inclination of the surface of the planet rather than to variations in its level, but if we could determine the inclination and knew the distance through which it extended, we should have all the data required for our work.

There is one conclusion however to which these observations lead us at once. Since these notches in the terminator do not necessarily occur in the darkest parts of the grey regions, and since different portions of them are notched to different depths

when on the terminator, it follows that all portions of the grey regions are not on the same level. In other words hills and valleys occur in them, and consequently the grey regions do not represent the surface of an ocean.

LOWELL OBSERVATORY, Flagstaff, Arizona.

July 13, 1894.

ON THE PERIODIC TIME AND DISTANCE OF THE FIFTH SATELLITE OF JUPITER.*

E. E. BARNARD.

This satellite has been observed at every favorable opportunity during the opposition of 1893. On that occasion it was first seen September 3d and was last observed 1894, January 28.

These observations, combined with those of 1892, now give a very accurate determination of the periodic time of the satellite.

The following values of the period have been obtained by comparison with the east elongation of 1892, September 10d 12^h 48^m.2 Standard Pacific Time.

STANDARD PACIFIC TIME.

East Elongation.				
1893 Oct.	1	16 ^h 11 ^m .84	Period = 11 ^h 57 ^m 22 ^s .615	
	2	16 5 .61		22 .554
Nov.	6	12 54 .54		22 .650
	12	12 20 .88		22 .607
	19	11 42 .98		22 .662

From these we have

$$\text{PERIODIC TIME} = 11^{\text{h}} 57^{\text{m}} 22^{\text{s}}.618 \pm 0^{\text{s}}.013$$

The recent observations seem to confirm the suspected eccentricity of the orbit.

The mean of the observed east elongations of 1892 from September to November was

$$48''.104 = 112650 \text{ miles}$$

while the mean of those for 1893-4 from Sept. to January is

$$47''.785 = 111910 \text{ miles}$$

These are reduced to distance 5.20.

This last fairly corresponds to the West elongations of 1892,

* Communicated by the author.

viz. $47''.712$. M. Tisserand has found that the major axis of its orbit must make a complete revolution in five months.

No opportunity has yet occurred for a direct comparison of the light of the satellite with a star, so that its magnitude is still somewhat uncertain. It is scarcely possible that the brightness is greater than 13 magnitude—if it is as bright as that. So the assumed value of about 100 miles for its diameter should still be considered as not far from its actual size.

No eclipses of the satellite have been observed, and it is scarcely possible that they ever will be with our present instrumental equipment, as they will occur too close to the limb of Jupiter for the satellite to be seen.

Its shadow has not yet been seen on the surface of Jupiter and doubtless does not reach the planet unless as a point too small to be seen with our present telescopes. During the observations of this object in 1892 and 1893 a series of filar micrometer measures of the diameters of Jupiter were made for the homogeneous reduction of the observations.

From these the following values result. They are also reduced to distance 5.20

Equatorial Diameter	$38''.522 \pm 0''.024$ (34 nights)
Polar Diameter	$36''.112 \pm 0''.032$ (24 nights)

which correspond respectively to

	90194 ± 56 miles
and	84566 ± 75 miles

In comparing these with previous measures, it was found that filar micrometer measures of the diameters of Jupiter are uniformly about $1''$ greater than those made with heliometers. The complete observations of the satellite, which were all made with the 36-in., will be published in the *Astronomical Journal*.

MT. HAMILTON, July 7, 1894.

PRELIMINARY NOTE ON THE OBSERVATIONS OF SATURN AND
URANUS WITH THE 36-in. EQUATORIAL.*

E. E. BARNARD.

SATURN.

Since the first of the year I have been engaged in the work of re-measuring the ball and ring system of Saturn with the 36-

* Communicated by the author.

inch, a work especially suited for our great telescope. Among the other points in view in this work was a series of measures made with the intention of detecting any displacement of the ball from the center of the ring.

The measures were made from the ends of the ring to the nearest limb of the planet.

The results so far obtained from ten nights' measures, show a slight difference that would make the distance on the following side a little greater than on the preceding side. This may be due to some peculiarity in the measures themselves as the quantity is less than $0''.1$.

The values reduced to the mean distance of Saturn are:

From <i>p</i> end of Ring to <i>p</i> limb of Saturn.	From <i>f</i> end of Ring to <i>f</i> limb of Saturn.
11''.197	11''.287

From ten nights' measures.

The difference, $0''.090$, would scarcely suggest any apparent deviation of the ball from the exact center of the rings.

The dimensions of all the rings and the ball are also being carefully measured. A preliminary reduction of the work shows the results will agree very closely with those obtained by Hall with the 26-inch at Washington some 10 or 15 years ago. (See "Saturn and its Ring," Washington Observations, 1885, Appendix II.)

URANUS.

Besides a careful series of measures of the positions of the satellites of Uranus for a more accurate re-determination of the mass of that planet, I have also made a series of measures of its polar and equatorial diameters, and a series of measures of the position angle of its equator. The ellipticity of the disc is quite noticeable.

The following are the position angles so far obtained :

	P. A. of Equator.
1894 April .29	11.3
" 30	16.8
May 6	19.5
" 7	12.3
" 21	7.0
" 28	7.1

From these it would appear that the equator of the planet essentially coincides with the planes of the orbits of the satellites, verifying the supposition that Uranus rotates on an axis deviating but little from the plane of its orbit.

The observations of the satellites extend from April 16.

The complete observations of both these planets will be published when the observations are finished.

MT. HAMILTON, June 9, 1894.

ORBIT OF THE BINARY STAR OΣ 224.*

J. E. GORE.

Although the angular motion of this binary pair has been only about 60° between the years 1843 and 1892, I find that the motion has been round the apoastron end of the apparent ellipse, and that consequently the period of revolution will be shorter than the angular motion would at first sight seem to indicate, and shorter than that found by Professor Glasenapp in *ASTRONOMY AND ASTRO-PHYSICS*, No. 118. Using the measures given by Mr. Burnham in *ASTRONOMY AND ASTRO-PHYSICS*, No. 108, I have drawn the apparent ellipse from an interpolating curve, and from this ellipse, I obtained, by Professor Glasenapp's method, the following values of the coefficients in the general equation of the second degree:

$$\alpha x + \beta y + \gamma x^2 + \delta xy + \varepsilon y^2 + 1 = 0.$$

$$\begin{array}{ll} \alpha = + 0.0136 & \delta = - 0.0001501 \\ \beta = - 0.020624 & \varepsilon = - 0.000526 \\ \gamma = - 0.0002174. \end{array}$$

Substituting these values in Kowelsky's equations, I have obtained the following provisional elements for OΣ 224.

$$\begin{array}{ll} P = 96.13 \text{ years.} & \lambda = 316^\circ 3' \\ T = 1833.25 & i = 49^\circ 46' \\ e = 0.579 & a = 0''.42 \\ Q = 179^\circ 14' & u = -3.7447 \end{array}$$

Measured in the direction of the star's motion, which is retrograde, λ would be $43^\circ 57'$.

The following is a comparison between the measures used in calculating the orbit and the positions computed from the above elements. Some of the measures are rather discordant.

* Communicated by the author.

Epoch.	Observer.	θ_0 °	θ_c °	$\theta_0 - \theta_c$ °	ρ_0 "	ρ_c "	$\rho_0 - \rho_c$ "
1843.22	Mädler	13.7	20.3	- 6.6	0.35	0.30	+ 0.05
1844.31	O. Struve	20 ±	14.3	+ 5.7	—	0.33	—
1845.30	Mädler	13.6	13.3	+ 0.3	0.20	0.35	- 0.15
1851.27	O. Struve	352.6	361.0	- 8.4	0.48	0.46	+ 0.02
1851.28	Mädler	17.5	1.0	+ 16.5	0.25	0.46	- 0.21
1857.34	Secchi	3.6	352.6	+ 11.0	—	0.53	—
1861.26	O. Struve	348.8	348.1	+ 0.7	0.59	0.56	+ 0.03
1868.03	Dembowski	339.2	341.0	- 1.8	0.5 ±	0.59	- 0.09
1871.31	O. Struve	328.4	337.6	- 9.2	0.59	0.59	0.00
1872.31	" "	336.8	336.7	+ 0.1	0.55	0.59	- 0.04
1873.23	Dembowski	329.8	335.8	- 6.0	—	0.59	—
1879.32	Schiaparelli	315.7	329.5	- 13.8	0.35	0.57	- 0.22
1880.16	Burnham	334.3	328.6	+ 5.7	0.62	0.57	+ 0.05
1881.25	Doberck	316.6	327.4	- 10.8	—	0.56	—
1882.27	"	309.9	326.3	- 16.4	—	0.56	—
1883.71	Engelmann	330.2	324.6	+ 5.6	0.53	0.55	- 0.02
1884.21	Perrotin	326.0	324.1	+ 1.9	0.55	0.55	0.00
1887.27	Schiaparelli	315.6	320.4	- 4.8	0.52	0.53	- 0.01
1892.37	Burnham	313.6	313.4	+ 0.2	0.48	0.49	- 0.01

The following are the formulæ for computing the position-angle and distance at any given time t :

$$(1). \quad u - 33.17 \sin u = -3.7447(t - 1833.25)$$

$$(2). \quad \tan \frac{1}{2}V = [0.2870500] \tan \frac{1}{2}u$$

$$(3). \quad \tan(\theta_c - 179^\circ 14') = [9.8101666] \tan(V + 316^\circ 3')$$

$$(4). \quad \rho = 0''.42(1 - 0.579 \cos u) \frac{\cos(V + 316^\circ 3')}{\cos(\theta_c - 179^\circ 14')}$$

The figures in brackets are logarithms.

I have computed the following ephemeris for comparison with those given by Professor Glasenapp:

t	θ_c °	ρ_c "	t	θ_c °	ρ_c "
1895.30	308.9	0.46	1915.30	250.9	0.27
1900.30	299.7	0.41	1920.30	223.7	0.24
1905.30	287.9	0.36	1925.30	190.2	0.21
1910.30	272.2	0.31	1930.30	130.3	0.13

Assuming that the mass of the system is equal to the mass of the Sun, the hypothetical parallax would be

$$p = \frac{a}{p^{\frac{2}{3}}} = 0''.02.$$

BALLYSODARE, County Sligo, Ireland.

PLATE XVIII.



GALE'S COMET,

1894. MAY 5, 8^h 45^m — 11^h 15^m S. P. T.

By E. E. BARNARD.

(See article in June number.)

Astro-Physics.

ON THE SPECTRUM OF β Lyræ.*

H. C. VOGEL.

In conclusion I give a brief review of the more recent observations which have been made elsewhere.

From the short notice which Pickering published in *Astr. Nach.* 3051, it appears that the twenty-five plates of the spectrum of β Lyræ, which were taken in the course of more than four years, agree as to the most important points with the observations here given. He distinguishes, as we also were led to do, between the appearance of the lines in the first and that in the second half of the light period from one principal minimum to another. He says:

"Of the eleven plates in which the bright lines had a diminished wave-length, it was found that all had been taken during the second half of the period of variation, that is, after the second minimum and more than $6d\ 11^h$ after the principal minimum. The fourteen plates taken during the first half of the period all showed an increase in wave-length of the bright lines; that is, the dark lines appear bright on the side toward the red." Pickering further says: "The actual changes in the spectra when studied in detail are much more complicated than has been stated above, and show a variety of intermediate phases, and changes in the dark as in the bright lines. In some of the photographs several of the bright lines appear to be double."

Under the assumption that the object is a close double star whose components possess different spectra and whose period of revolution equals the light period of 12.9 days, Pickering deduces from the observed displacement of the lines a velocity of 480 kilometres per second, and calculates the radius of the orbit, which is assumed circular, to be eighty million kilometres. The determination of the velocity was made, without doubt, from the measurements, taken at the time of a principal minimum or first maximum, of the bright and dark lines which lie apparently near each other. As a result of the use of the object-glass prism, and the consequently diminished sharpness of the spectra as compared with photographs taken with the slit spectroscope, the distance observed by Pickering is considerably greater than that

* Continued from page 367.

found here. If the calculation is carried further, it leads to an enormous mass ($130 \odot$) for the system. Also, if the more probable assumption is made, that the centre of mass of the system falls, not at the center of one star, but between the two, it follows that the mass of the system is $16 \odot$, which is still extremely improbable. But just as I have shown above that it is not allowable, without further information, to regard the distance between the centres of the bright and dark lines as the amount of the displacement, so it is not consistent with the observed phenomena to assume that the orbit is circular.

Pickering's important notice concerning the double spectrum of β Lyræ, which up to that time had been unknown, appeared in June, 1891; and it closes with the following remarks as to further hypotheses, which, however, I cannot support, because they seem to offer no sufficient explanation of the complicated phenomena in question:

"The phenomena may also be due to a meteor stream or to an object like our Sun, revolving in $12d\ 22^h$ and having a large protuberance extending over more than 180° in longitude. The occasional doubling of the lines would then be due to both ends of the protuberance being visible at the same time, one receding, the other approaching. The variation in light may be caused by the visibility of a larger or smaller portion of the protuberance."

Observations were made by J. E. Keeler* at the Lick Observatory during the year 1889. They were direct observations with the spectroscope, and refer to the visible spectrum between C and F. Keeler's results, in his own opinion, were not sufficient to give a comprehensive and at all satisfactory explanation of the observed phenomena; and on this account he intended to make further observations, which however was not done owing to his withdrawal from the Observatory. His preliminary results were published only because in his opinion it was improbable that they would ever be continued, as in the mean time it had become apparent that direct observations could not in general compare with those based upon the newer photographic methods even when the latter are made with telescopes of inferior power. The results are in my opinion not without importance, although they differ in many points from the deductions made from the photographic observations in other parts of the spectrum; and I quote them in Keeler's own words:

1. In the spectrum of β Lyræ the bright hydrogen lines C and F, the bright D_3 line, and the dark D lines are always visible with

* A. AND A.-P. April, 1893, p. 114.

a telescope as large as the Lick refractor. Certain fainter bright lines are visible except at the time of a principal minimum.

2. The variations in the light of the star are principally due to changes in the brightness of the continuous spectrum.

3. The bright lines are brightest when the continuous spectrum is brightest. This is the case in most of the observations. Certain exceptions may possibly be real, in which case they would indicate either irregular variations of brightness, or a variation having a period different from that of the star, or they may be due to errors of estimation arising from the diminished brightness of the continuous spectrum at the time of a principal minimum.

4. The bright lines are broad and diffuse, particularly when the star is at a maximum. The D lines are very hazy, so that the components are hardly distinguishable.

5. During the greater part of the period of the star no remarkable changes occur in the appearance of the spectrum. The observations fail to show any connection between changes in the spectrum and the secondary minimum of the star.

6. The most remarkable changes take place at the time of a principal minimum. The bright lines become dimmer, and perhaps sharper. The fainter bright lines disappear. The D lines become darker. Strong absorption lines appear on the more refrangible side of certain bright lines in the green, the separation of the dark and bright lines being at least five tenth-metres. Other bright lines are perhaps similarly affected. A narrow dark line appears above the D_2 line at the same time. Shortly before the first maximum is reached the dark lines disappear."

The most complete observations on β Lyræ, which have been made up to the this time are those of Belopolsky.* They are of especial importance because the instrument used was the 30-in. Pulkowa refractor. Twenty-five spectrograms were obtained between Aug. 24 and Nov. 26, 1892. On account of the achromatising of the object glass for visual rays, Edward's orthochromatic plates were used, and the photographs are of the portion of the spectrum between D and H γ .

Belopolsky has made a special study of the F line (H β), the D_3 line, and the group at λ 447 $\mu\mu$ and 448 $\mu\mu$, and has given besides a table of all the lines which could be recognized in that portion of the spectrum lying between D and H γ . Accompanying the

* *Astr. Nachr.* 3129; *Memorie della Societa degli Spettroscopisti Italiani*, vol. XXII, p. 101. A further article in which the observations are published in detail has appeared, while this paper was in press, in Mélanges's *Math. et Astr.*, t. VII, line 3.

memoir are seven drawings of the spectrum as it appears at different phases of the light period. These are of interest especially in reference to the changes of the bright and dark F line, as they are in complete accord with the changes observed here in the $H\gamma$ line, which lies far in the violet. The description which Belopolsky gives of the changes in the F line agree very well with the observations [of the same line] made here at certain times. A translation of this description is as follows:

"At the time of the principal minimum, the bright line is single and lies on one side of a dark line. (At a considerable distance, quite by itself, there can be seen a second dark line.) At the time of the succeeding maximum, the bright line begins to become double, the component on the violet side being very narrow. During the second minimum, the bright line becomes double, and nearly symmetrical. During the second maximum, the appearance changes but slightly, the component on the red side becoming, however, narrower than the other. After this maximum* there appears on the outer edge of that component which lies towards the red a dark line."

This last statement is remarkable, because in the observations made here in case of the $H\beta$ line and $H\gamma$ line as well, there is noted the appearance of a dark line on the red side of the bright line at at the time of the second maximum.

From the drawing of Oct. 7 we get further confirmation of the observations which were made here, in that *after* the second maximum the component of the bright line towards the smaller wavelengths is decidedly broader than *at* the time of the second maximum according to the drawing of Oct. 2; and that 1.3 days *before* the principal minimum (Oct. 8.6) the appearance of the line is entirely different from that *at* the time of the minimum.

On the Pulkowa photograms, a comparison spectrum, hydrogen, iron or sodium, is photographed simultaneously with the spectrum of the star; and in this way it is possible to determine from fifteen plates the motions of the bright F line during the light-period of 12.9 days. The results are in surprising agreement with the period; and I give here the measurements themselves, to which I have added for comparison the times of the chief divisions of the phases of the light-variation.

* Belopolsky says, p. 104: "*Après ce maximum.*" Yet on p. 107 this dark line is further described, and we are told that it was seen on Sept. 8 and Nov. 25, 1892, that is one day before the second maximum, corresponding to the observations made here.

Time of Observation. 1892.	Phase.	Velocity Relative to Sun. Calculated from the Displacement of		Differ- ence, $b - d$
		the bright F line.	the dark F line.	
Sept. 23.3	22.5 II Max.	- 83 kil.	- 36 kil.	- 47
24.4		- 86	- 73	- 13
25.4	25.7 I Min.	- 32		
27.3	28.9 I Max.	+ 36	- 248	+ 178
30.3		+ 80	- 13	+ 118
Oct. 2.3	2.1 II Min.	+ 13	- 36	+ 49
3.3	5.4 II Max.	- 26	- 36	+ 10
7.3	8.6 I Min.	- 69	- 47	- 22
11.3	11.8 I Max.	+ 76	- 63	+ 139
19.3	18.3 II Max.	- 91	- 44	- 47
20.3		- 76	- 97	+ 21
22.3	21.5 I Min.	*	- 93	
26.3	24.8 I Max.	+ 79	- 66	+ 145
Nov. 25.2		- 53	- 47	- 6
26.2	26.1 II Max.	- 79	- 77	- 2

A positive motion in this table means a displacement towards the red. Belopolsky next deduces from his observations that at the times of the principal (or first) minimum and the second minimum that component of the velocity of the bright line, which lies in the line of sight = 0, while at the time of the first maximum it is nearly + 82, and at the time of the second maximum it is - 82 kilometres per sec. The displacement of the dark lines with reference to the artificial hydrogen F line was also determined; and I have included these measurements in the above table, placing in the last column the exponent of the relative motion of the two lines in the sense of bright line - dark line.

Unfortunately I am not in a condition to directly verify these measurements, because, as I have already mentioned we have not yet succeeded in satisfactorily carrying out measurements of the displacement of the lines in the stellar spectrum with reference to the lines in the spectrum of an artificial source. It is however noteworthy that all the values for the displacement of the dark line are negative, although there must be assumed some connection between it and the bright line; while for the bright line, variations are found towards the positive and negative side, which so balance each other that there is no evidence any motion of the hypothetical system itself.

We must note that unfortunately the observations are incomplete for the times of the principal minimum. Once, on Sept. 25, the displacement of the dark line is not measured; a second time, on Oct. 22, according to Belopolsky's description, the displacement of the bright line could not be measured, because its edge was covered (?) by the dark line. It is apparent from the description of the Pulkowa spectrograms as well as from the draw-

* "Un bord est convert par la raie sombre, impossible de mesurer."

ings made from the observations taken there, that at the time of the principal minimum and also the first maximum the distance between the bright and dark lines is the greatest. Accordingly we would expect to find for the bright line on Oct. 22 a strong positive motion of perhaps 75 kilometres, or at least as great as at the time of the next day of observations which falls after the first maximum, and therefore the regularity would be greatly disturbed, with which the observed points lie on the curve for the displacement,—a curve which is based on the exceedingly improbable assumption of a nearly circular orbit.

The measurements were moreover difficult, even with the powerful resources of the Pulkowa Observatory; so that considerable variations need not cause surprise. Besides I must once more mention the fact that the measurements cannot be regarded as entirely free from objection, since, on account of the partial superposition of the bright line on the absorption line, we should expect to find that the positions of the lines are reciprocally influenced.

In reference to the drawings, which were evidently made with great care, it is surprising that in the first three the bright and dark *F* lines in the stellar spectrum should appear in relation to the artificial line differently from the way they ought to according to the measurements.

Belopolsky believes that he finds an explanation of the phenomena in the assumption that a body whose spectrum gives the bright lines produces a partial eclipse of another body by passing over it at the time of the minimum. One ground for this assumption is that the continuous spectrum becomes weak at the time of the principal minimum, while the bright *F* line shows no corresponding decrease in intensity. From the observed maximum velocity of the motion of the bright lines of 89 kilometres and the period of 12.9 days, he calculates the radius of the orbit (assumed to be circular) to be in round numbers fifteen million kilometres; the mass of the system is then equal to that of the Sun.

In my opinion the time has not yet come for an exhaustive explanation of these very complicated phenomena, because I do not regard even the great mass of observations here given as sufficient for this purpose. Yet I do not question the fact that the important features of the variations in the lines can be shown to depend upon the motion of neighboring celestial bodies. It can, however, be settled only by further observations, whether or not the assumption of two bodies will suffice for an explanation of the fluctuations in the light, and the alterations in the spectrum so far as they refer to the lines.

As the existence of very close double-stars has been conclusively proved by means of the spectroscope, the peculiar changes in the light of β Lyræ can be explained by the close passage of two celestial bodies, one of which emits less light than the other, on the assumption of a nearly circular orbit, or an elliptical one whose major axis coincides with the line of sight and whose periastron is towards the Sun. When the dimmer of the two passes in front of the brighter and partially covers it, we have the principal minimum; the two equal maxima occur when the line joining the centres of the two bodies is perpendicular to the line of sight; the second minimum follows when the bright body partially conceals the dimmer one. On the other hand, the relative displacements of the lines can be explained by the revolution of two bodies, one emitting a bright line spectrum the other having an absorption spectrum, provided that the orbit deviates widely from a circle, and the major axis of the orbit makes a considerable angle with the line of sight. Both phenomena cannot be brought successfully under one assumption.

Returning again to Belopolsky's conception, I wish to call attention to the fact that if one of the bodies gives a bright-line spectrum, it must be assumed that the spectrum with the dark lines belongs to that body which is eclipsed at the time of the minimum. In this case, at the times of the principal and subsidiary minima, the components in the line of sight of the motions of both stars must vanish; the centres of the bright and dark lines must coincide. At the times of the the maxima, the component of the motion in the line of sight must have its greatest value, dark and bright must be relatively displaced, and in opposite directions at the two maxima. All this, however, is directly contrary to observation.

Father Sidgreaves has recently published* his observations on the spectrum of β Lyræ. They were made with a spectroscope without a slit, in connection with an 8-inch refractor. Forty-five plates in all were taken, ten in the spring of 1892, the rest in May and August, 1893. The time of exposure is not definitely stated, yet it follows from the description that it must have been long; and that most of the poor plates were the result of under exposure. No essentially new points of view are gained from these observations. They are confined mainly to the changes in the H γ line and the lines at $439\mu\mu$ and $447\mu\mu$, since other portions of the spectrum were too diffuse or too weak to permit a study of the details. This much, though, can be noted, that on the plate

* *Monthly Notices* Vol. LIV, p. 96.

which accompanies his paper and in which are given the changes in the lines from day to day throughout the light period, the $H\beta$ line, lying at one limit, is always represented as bright, the $H\delta$ line, lying at the other limit, is always represented as dark. I might observe further that our observations here show that it is hardly allowable to combine, as Sidgreaves does, observations made months apart, in order to show the changes in the lines during the light-period. However the observations of Sidgreaves may assume greater importance when they are published in a more detailed form.

NOTE ON THE SPECTRUM OF THE GREAT NEBULA IN ORION.*

WILLIAM HUGGINS.

With reference to Professor Campbell's observations on the spectrum of the Orion nebula (p. 384) it may be well for me to state at once that the photographs taken 1888-1890 are still in good condition, and fully justify the interpretation we put upon them at the time. We regret very much that the small scale of the photographs, and the delicacy of some of the details make it impossible to reproduce them, in a sufficiently adequate manner, for publication.

It was our intention to work on this nebula last autumn, but the spectroscope designed for this work was not completed in time. We hope to do so next season, and will give special attention to the points on which Professor Campbell's recent observations differ from our earlier ones.

As Professor Campbell's remarks on the broadening of certain portions of the lines upon our plates (pp. 391-393) seem to show that he has not understood correctly the interpretation we put upon this appearance, I may say now that the view we took, and still hold is that this broadening is purely a photographic spreading on account of greater brightness of the line at that place. This greater brightness might be due to more energetic radiation, but is to be attributed more probably to radiation from a large number of molecules in consequence of a greater depth of the nebula in the line of sight, or of local condensation, at these places.

Further we suspected that the strong photographic line at 3727 may vary in brightness relatively to the hydrogen lines, at

* Communicated by the author.

different points of the nebula, in a manner similar to the known variation in the visible region of the principal line to the line of hydrogen at F.

In the construction of a spectroscope for taking photographs of the spectra of stars as early as 1876 (*Phil. Trans.* 1880, p. 670).

I reduced the time of exposure by using a short camera with a lens of $6\frac{1}{2}$ -inches focal length and a ratio of nearly $\frac{f}{4}$. In one of the telescopes recently constructed the camera lens has half the focal length only of the lens of the collimator, namely 12 inches and an aperture $2\frac{1}{4}$ -inches. In the other instrument with a longer collimator, the camera lens has a focal length of $5\frac{3}{4}$ -inches only, and a ratio of $\frac{f}{4}$ nearly.

THE TEMPERATURE OF THE SURFACE OF THE FIXED STARS
AND OF THE SUN, COMPARED WITH THAT OF TERRESTRIAL
SOURCES OF HEAT.*

J. SCHEINER.

In the course of my investigations on the spectra of the brighter stars, with the aid of photographs taken at the Potsdam Observatory, I was struck by the peculiar behavior of a line (λ 4482) which belongs to the spectrum of magnesium. In nearly all spectra belonging to class I this line is prominent, either on account of its breadth or its intensity; in spectra of this class which contain four lines, it even equals the hydrogen lines in width. It is also very prominent in the spectra of Sirius, Vega, Procyon and other stars in whose spectra lines are more abundant, although not in the same degree as in those first mentioned; on the other hand it is weak in the solar spectrum and in the other spectra of class II—indeed in some representatives of this class it does not appear,—and it seems as if this line becomes weaker the closer the spectrum approaches to Class III.

In the spectrum of magnesium when produced artificially this line is also subject to great variations in breadth and intensity. It cannot be recognized in the spectrum of the magnesium flame, or in that of magnesium vapor in the electric arc, but it reaches a very great intensity and breadth in the spark spectrum. Living and Dewar† have called attention to this behavior of the line, and

* Translated from the *Sitzungsberichte der k. preuss. Akademie der Wissenschaften zu Berlin*, March, 1894.

† *Proc. Roy. Soc.*, XXX, p. 93.

the investigations of Kayser and Runge, as well as my own observations, are in confirmation of their statements.

It is of course natural to ascribe the peculiarities of the line to differences of temperature of the magnesium vapor in the electric arc and in the spark, and therefrom to draw further conclusions as to the temperature of the fixed stars; it is however impossible to sharply separate the influence of temperature from that of pressure, and the only allowable conclusion with reference to the stars is, that magnesium vapor in stars of Class I is in the same condition as in electric sparks of high tension, and in stars belonging to Class II it is in the same condition as in the electric arc.

But it is remarkable that another line of the magnesium spectrum (λ 4352) behaves, according to my observations, in just the contrary manner to that described above. It does not appear in any of the spectra of Class I containing four lines, but begins to be visible in richer spectra of this class: is very prominent in the Sun and stars of Class IIa, and in α Orionis (Class IIIa) is one of the strongest lines in the spectrum. In laboratory experiments also, this line has just the reverse appearance of that at λ 4482. In the spark spectrum it is hardly if at all recognizable, while in the arc spectrum it is very strong and broad. Living and Dewar have also noticed this peculiar character of the line.

The favorable circumstances that two lines belonging to the same substance have this opposite behavior, proves at once that the appearances presented by these lines in the stars depend only on the temperature and not on the pressure. With increased pressure all the lines of a gas become broader and more prominent; it cannot happen as a consequence of Kirchhoff's law that a line can narrow with increased pressure. On the other hand it is a well-known fact that single lines may become weaker and narrower at higher temperatures, although in general lines under these conditions became stronger and broader. I believe therefore that I am justified in making the following statement:

The temperature of the so-called reversing layer—the outermost layer of the photosphere—in stars of class IIIa is approximately equal to that of the electric arc (*about* 3000° or 4000° C.); in the Sun, and in stars of class IIa the temperature is higher, but does not reach that of the spark of a Leyden jar; in stars of class Ia it is approximately equal to the temperature of this spark (upper limit about 15000° C).

With this result is also given, for the first time, a direct proof of

the correctness of the physical interpretation of Vogel's spectral classes, according to which class II is developed by cooling from I, and III by a further process of cooling from II.

NOTE ON THE SPECTRA OF COMETS *b* and *c* 1893.*

W. W. CAMPBELL.

Professor Kayser, in a *Note on the Spectra of Comets* in the March number of this journal based upon my observations† of Comet *b* 1893, refers to my comparisons of the comet bright-line spectrum with the artificial spectra of carbon and cyanogen only by saying that I have "committed some mistakes." I have read Professor Kayser's note with great care (and considerable anticipation), and find the "mistakes" attributed to me are two in number. Now with perfect respect for Professor Kayser and his excellent work, I would point out that his use of the English expression "committed some mistakes" is unfortunate and misleading.

One of the "mistakes committed" is:

(a). "Campbell supposes that this part of the spectrum [of carbon and cyanogen, between λ 436 and λ 423] has not been covered by our work, but here he is mistaken." This is not a scientific point, and must not be discussed by me. I can only judge of what has been done by what has been published. My reasons for saying, "The region λ 436- λ 423 does not appear to have been covered by the work of Kayser and Runge," were taken from their publication,‡ and are as follows:

I. In describing the spectrum they say, on page 5, " * * * no part of the spectrum between $\lambda = 620\mu\mu$ and $\lambda = 340\mu\mu$ is free from carbon lines; there are certainly over 10,000 of them."—This led me to suppose that the region λ 436- λ 423 contains numerous lines. Further, Angström and others place the limits of the fifth carbon band at λ 438 and λ 423; and H. W. Vogel's photographs of cyanogen-flame and Bunsen-flame spectra show a number of very prominent lines in the region 436- λ 423.

2. In the next paragraph, page 6, they say, "We naturally did not plan to measure the wave-lengths of all these lines; we confined ourselves indeed to giving in detail the second, third and fourth cyanogen bands (at λ 422, λ 388, λ 359;) likewise, as an

* Communicated by the author.

† Described in A. & A.-P. for August, 1893.

‡ In *Abhandlungen d. Berlin. Akad.*, 1889.

example of a carbon band, the green band which begins at λ 516; and finally, for reasons which will appear later, the beginning of the band at λ 474."—It will be seen that *the region λ 436- λ 428 is not included in their program of work.*

3. There are five bands in the spectrum of carbon. They publish three wave-lengths in the second band, and verbally describe the first and second bands on page 8. Their measures and photographs describe the third and fourth bands perfectly. Each of the first four bands contains a large number of lines. They publish three wave-lengths in the fifth band (4382, 4371, 4365), and I did not find further information concerning this band in any part of their paper. For the region λ 436- λ 423, constituting nearly the whole of the fifth band, no photographs, nor wave-lengths, nor verbal descriptions, are given. It seemed to be a part of the carbon spectrum not described by them in any manner whatever.

The other "mistake" is this:

(b). "Campbell erroneously compares the first three of these lines [meaning 4098, 4073, 4052] with lines measured by us in the second group of cyanogen, which are much too weak to have been seen in the comet."—Their published photographs do not cover all this region; and from their list of wave-lengths I selected three lines because they occupy the proper positions for coinciding with three observed comet lines and are marked "*stark*" (that is, strong or relatively bright). If Professor Kayser holds that they are much too faint to appear in the comet, I readily agree that the comparison at that point is of small weight; but it does not constitute a positive "mistake."

In all other points Professor Kayser arrives at identically the same conclusions reached by me last year,* except that he gives one additional coincidence at λ 509 (with Angström and Thalen's 5098 carbon line), and ascribes the comet lines λ 4366, λ 4313 without doubt to two bands of burning hydro-carbon, whereas I attributed them to possible lines in the flame spectrum of cyanogen. The subject is of sufficient importance to warrant the reproduction of my table of comparisons, and include in it the results just offered by Professor Kayser. His acknowledged authority on carbon and other spectra gives these results great weight, and his closing sentence contains an hypothesis which, if finally confirmed will be most important.

* In A. AND A.-P. for August, 1893, and more complete comparisons in *Publ. Astr. Soc. Pac.* for Sept. 1893.

Comet lines—visual observations.		Identifications.	
Wave- Lengths	Description.	Campbell.	Kayser.
600	Max. of red band, broad, faint.....	619-595 carbon	619-595* carbon
5633	Very faint line, edge yellow band...	5635 "	5635 "
558	Bright line in yellow band.....	5585 "	5585 "
5163	Very bright line, edge green band..	5165 "	5165 "
5126	Very bright line in green band.....	5129 "	5129 "
509	Very bright line in green band.....	— "	5098* "
4734	Bright line, edge blue band.....	4737 "	4737 "

Comet lines—photographic observations.		Identifications.	
Wave- Length	Description.	Campbell.	Kayser.
4736	Very bright line, edge blue band.....	4737 carbon.	4737 carbon.
4716	Very bright line in blue band.....	4715 "	4715 "
4697	Very bright line in blue band.....	4698 "	4698 "
4683	Very bright line in blue band.....	4685 "	4685 "
4675	Apparently very bright line, not clearly separated from 4683.....	4676† "	4677‡ "
4366	Very bright line.....	Cyanogen flame?	Burning hydro- carbon.
4350	Very faint.....	—	—
4335	Faint line.....	—	—
4313	Very bright line.....	Cyanogen flame?	Burning hydro- carbon.
4298	Very bright line.....	—	—
4253	Very faint line.....	—	—
4234	Very faint line.....	—	—
4214	Very bright line.....	4216 cyanogen	4216 cyanogen
4196	Bright line.....	4197 "	4197 "
4178	Very faint line, uncertain.....	4181 "	4181 "
4126	Faint line.....	4128 "	—
4098	Bright line.....	4099 "	—
4073	Bright line.....	4074 "	—
4052	Bright line.....	4053 "	—
4043	Bright line.....	—	—
4019	Bright line.....	—	—
4011	Faint line.....	—	—
3988	Very faint line.....	—	—
3881	very bright line, probably brightest in spectrum.....	3883 cyanogen	3883 cyanogen
3870	Very bright line, resembles a band.....	3871 "	3871 "

Professor Kayser calls attention to the fact that a cyanogen group of lines at λ 461- λ 450 was not observed in this comet, perhaps owing to faintness, or because it might be covered by the end of the fourth carbon group. A careful re-examination of the

* Angstrom and Thalen.

† Swan, Hasselberg, Watts.

‡ Watts.

negatives has revealed no signs of its presence. But I probably observed that group visually in *Comet c* 1893. (The spectrum is described in *Publ., A. S. P.*, pp. 208-10). In this comet, in addition to the three carbon bands usually present, I observed two bands possibly not hitherto seen: one at $\lambda 455$ which I attributed to the cyanogen group of lines shown in H. W. Vogel's photographs, and a narrow band or line near $\lambda 4863$, which may possibly be the $H\beta$ hydrogen line. The green and blue carbon bands in this spectrum were much narrower, and more suddenly terminated on the more refrangible side than usual. The two extra bands, in addition to the usual three, were observed on four nights. It was not possible to get photographs of the spectrum.

LICK OBSERVATORY, 1894, March 20.

ON THE PHOTOGRAPHIC SPECTRUM OF THE GREAT NEBULA IN ORION.*

J. NORMAN LOCKYER, C. B., F. R. S.

The paper consists of a description and discussion of photographs of the spectrum of the Orion Nebula, taken with the 30-inch reflector at Westgate-on-Sea in February, 1890, of which a preliminary account was communicated to the Royal Society at the time. Fifty-four lines are tabulated as belonging to the spectrum of the nebula, nine of them being due to hydrogen. Tables are given showing:

1. The wave-lengths, intensities, and probable origins of the lines photographed in the spectrum of the nebula.
2. A comparison of the lines in the spectrum of the nebula with lines in the spectra of (a) *P. Cygni*, (b) bright line stars and planetary nebulae, and (c) stars in Groups II, III, and IV, of the classification according to the meteoritic hypothesis.

The complete discussion has led to the following general conclusions:—

1. The spectrum of the nebula of Orion is a compound one consisting of hydrogen lines, low temperature metallic lines and flutings, and high temperature lines. The mean temperature, however, is relatively low.†

* Abstract of a paper read before the Royal Society. Communicated by the author.

† *Roy. Soc. Proc.* Vol. 43, p. 152, 1887.

2. The spectrum is different in different parts of the nebula.
3. The spectrum bears a striking resemblance to that of the planetary nebulae and bright line stars.
4. The suggestion, therefore, that these are bodies which must be closely associated in any valid scheme of classification is confirmed.
5. Many of the lines which appear bright in the spectrum of the nebula appear dark in the spectra of stars of Groups II and III; and in the earlier stars of Group IV, and a gradual change from bright to dark lines has been found.
6. The view, therefore, that bright line stars occupy an intermediate position between nebulae and stars of Groups II and III is greatly strengthened by these researches.

THE SPECTRUM CHANGES IN β Lyræ. PRELIMINARY NOTE.*

J. NORMAN LOCKYER, C. B., F. R. S.

The spectrum of this well known variable star was first investigated photographically by Professor Pickering, at Harvard College Observatory, and a preliminary account of the results was published in 1891.† Dark and bright lines were found to be associated in the spectrum, and further, the bright lines were found to change their positions with respect to the corresponding dark ones according to the interval of time which had elapsed since the preceding minimum.

It may be remarked that the period of the light-changes of the star is about twelve days twenty-two hours, and there are two approximately equal maxima of mag. 3.4, a principal minimum of mag. 4.5, and a secondary minimum of 3.9, the period of variation stated being that which elapses between two successive principal minima.

Professor Pickering found that during the first half of the period—that is, between principal and secondary minima—the bright lines were on the less refrangible sides of the corresponding dark ones, while during the second half they were displaced to the more refrangible sides. He further remarked that “the actual changes in the spectra, when studied in detail, are much more complicated than has been stated above, and show a variety of intermediate phases and changes in the dark as well as in the bright lines.”

* Read before the Royal Society. Communicated by the author.

† *Ast. Nach.*, 2707; *Observatory*, 1891, p. 341.

At Professor Pickering's request, I took up the work at Kensington in July, 1891, the instrument employed being the 6-inch Henry object glass and prism of $7\frac{1}{2}^\circ$, which I have described in a previous communication.* Several photographs were taken with this instrument, but it was not until the new 6-inch prism of 45° † was employed in the research that any considerable advance was made. With the higher dispersion of this instrument the spectrum is depicted in greater detail, and more minute changes can therefore be determined.

Since my work was commenced, accounts of the photographic spectrum of β Lyræ have been published by Belopolsky,‡ Father Sidgreaves,§ and Vogel,|| and various suggestions have been made by them and others as to the conditions which bring about the variability.

On this account, although the reductions of the sixty-four photographs which I have obtained are not yet completed, I have thought it desirable to give a brief *résumé* of the facts already acquired.

For the complete study of the problem more photographs will be required, and a considerable amount of time will be required for the discussion of them. The present communication, therefore, is limited to a preliminary consideration of the variation in the spectrum as photographed at Kensington, and I have consequently in it omitted reference to the results obtained by other workers. In a subsequent paper, however, a complete history of the subject will be given.

To facilitate references to the spectrum, thirteen photographs—roughly one for each day of the period—are reproduced in Plate 1. These have been enlarged about three times from the original negatives.¶

As it is a matter of great difficulty to mount a series of such photographs showing the exact coincidences of the lines, in comparing the different spectra in the plates, some allowance must be made for the slight differences in scale. Further, it is right to add that probably some of the fainter lines shown in the photographs are artificially produced by the process of enlargement, but the real lines will be readily identified by their appearance in more than one spectrum; the lines of particular interest are indicated in Plate 2.

* *Phil. Trans.*, 1893, vol. 184, p. 678.

† *Ibid.*, p. 679.

‡ *Mem. Soc. Spett. Ital.*, June, 1893.

§ *Monthly Notices*, R. A. S., 1894, p. 96.

|| *Sitzungsberichte*, Berlin, February, 1894.

¶ The plates are omitted.

The light curve which forms part of Plate 1 is constructed after Argelander's drawing,* and the dotted lines drawn from the spectra to the period scale indicate the relation of each photograph to the light curve.

I proceed to state, step by step, the results of the preliminary examination of the photographs, and to indicate the spectral phenomena on which they are based.

1. *The spectrum is constant at the same interval from principal minimum.*

Apart from the slight differences, which seem to be accounted for by differences in the atmospheric conditions and consequently in the quality of the negatives, the spectrum appears to be the same at the same interval from minimum. The photographs reproduced in Plate I have been selected as being specially suitable for reproduction, but at most of the phases duplicates which are practically identical have been obtained.

2. *The kinds of variation shown on the photographs are as follows :*

- (a) Periodical changes in the relative intensities of the lines.
- (b) Periodical doublings of some of the dark lines.
- (c) Periodical changes in the positions of the bright lines with respect to the dark ones.

3. *There are two bodies involved giving dark line spectra.*

On reference to Plate I it will be seen that at, and just before and after the second maximum, some of the dark lines are doubled. This indicates two sources of light giving dark line spectra and moving relatively to each other in the direction of the line of sight. When the relative movement in the line of sight is zero, none of the lines are doubled. The latter condition occurs about the time of the two minima.

4. *The maximum relative velocity of the two dark line components in the line of sight is about 156 miles per second.*

The greatest separation of the dark lines occurs about the time of second maximum, and the relative velocity, as determined by measurements of three of the doubles in the photograph of August 24, 1893, is that stated above. The individual measurements are as follows :

$$\begin{array}{rcl} \text{H}\gamma & = & 155 \text{ miles per second.} \\ \text{H}\delta & = & 154 \quad \text{"} \\ \lambda 4025 & = & 158 \quad \text{"} \end{array}$$

5. *One of the dark line components bears a strong resemblance to Rigel and the other to Bellatrix.*

* De stella β Lyrae Disquisitio.

The spectra of the two components can readily be separated, for the reason that only lines common to both will be doubled. Among these are the lines of hydrogen. Lines special to either component are always single, and they retain the same relative positions with respect to one group of hydrogen lines throughout the period.

In Plate 2 photographs are given to facilitate an analysis of the compound dark-line spectrum. At the bottom of the diagram is a reproduction of a photograph taken near the time of second maximum (August 24, 1893), and the spectra of Rigel and Bellatrix are included in the same plate. The compound character of the dark line spectrum of β Lyrae at this time is shown by the fact that one group of lines corresponds very closely with those which appear in the spectrum of Rigel and when these are subtracted from the whole spectrum, a spectrum closely resembling that of Bellatrix remains, the latter spectrum being displaced in this photograph to the more refrangible side, as shown by the short lines drawn beneath the spectrum. The resemblance of the two components to Rigel and Bellatrix respectively, the spectra of which I have described in a previous paper,* is further shown by the following tabular comparison, the two dark line components of β Lyrae being called R and B respectively.

Component R.	Rigel.		Component B.	Bellatrix.	
Wave-Length.	Wave-Length.	Intensity.	Wave-Length.	Wave Length.	Intensity.
				3919	2
				3926	3
3933	3933	6	3933	3933	3
	3963	2	..	3963	3
3968	3968	6	3968	3968	6
3994	3994	1	..	3994	3
4008	4008	2	..	4008	5
4025	4025	3	4025	4025	6
				4040	2
				4069	2
				4071	2
				4075	2
4101	4101	6	4101	4101	6
				4104	2
				4119	2
4120.5	4120.5	2	4120.5	4120.5	4
4127	4127	3			
4130	4130	3			
4143	4143	2	4143	4143	5
				4168	3
	4172	1	..	4172	1
	4177	1	..	4177	1

* *Phil. Trans.*, 1893, vol. 184, p. 693.

Component R.			Component B.		
Rigel.			Bellatrix.		
Wave-Length.	Wave-Length.	Intensity.	Wave-Length.	Wave-Length.	Intensity.
4233	4233	2		4241.5	2
				4253	2
4267	4267	2		4267	4
4340	4340	6	4340	4340	6
				4345	2
4351	4351	1	4351	4351	2
4388	4388	3	4388	4388	5
				4394.3	2
				4414.5	2
				4417	2
				4437	3
4471	4471	4	4471	4471	6
4481	4481	5	4481	4481	3
				4553	3

It is not intended to suggest that the spectra of the two dark-line components are quite identical with those of Rigel and Bellatrix. These are simply the best known stars which they most closely resemble, and the similarity is pointed out as an indication that we have not to deal with bodies of an unfamiliar type. Throughout the paper I shall refer to the two components as R and B respectively.

The conditions at first maximum, as shown in Plate I, are not so simple as those at second maximum, though there is evidence to show that at this point of the light curve the component B is receding with respect to R. As will be seen on reference to the photograph of March 13, 1894, the hydrogen lines are broadened, and the two lines near 4471 and 4481 have approached each other, as they should do if one belongs more especially to R and the other to B.

6. *When the two bodies lie along the line of sight, partial eclipses occur. This happens near the minima of the light curve.*

The differences in the intensities of the dark lines special to R and B, near the two minima, indicate that near the principal minimum R is partially eclipsed by B, while near the secondary minimum B is partially eclipsed by R. These changes will be seen on Plate 1, again in Plate 2. In the latter we have comparisons of β Lyræ at the two minima, with Bellatrix and Rigel. If we leave the bright lines out of consideration, it will be seen that near principal minimum, the spectrum of β Lyræ greatly resembles that of Bellatrix, the component B in this case lying between us and component R. As the eclipse is not total, however, the

lines special to R appear with reduced intensities; the lines joining the spectrum of β Lyrae to that of Bellatrix indicate the principal lines of component B. At the secondary minimum, on the other hand, component R lies in front of component B, and the spectrum consequently bears a greater resemblance to that of Rigel. This is shown by the lines joining those of β Lyrae to the spectrum of Rigel in Plate 2.

The difference is especially noticeable in the case of the lines near λ 4471, 4481, 4388, and in the group of four lines a little less refrangible than H δ . It will be seen that near principal minimum 4471 is stronger than 4481, as in Bellatrix, while about secondary minimum 4481 is stronger than 4471.

If the eclipses were total, the variations of the spectrum might be expected to be still more striking.

7. *In addition to dark lines there are several bright ones, which change their positions with respect to the dark ones.*

The photographs show conspicuous bright lines about wavelengths 4862 (H β), 4715, 4471, 4388, 4340 (H γ), 4101 (H δ), 4025, and 3887 (H ϵ). Other fainter ones also appear in some of the best photographs. The line at 4471 (Lorenzonis *f*) is the well-known line which appears in the spectrum of the solar chromosphere, and those at 4025 and 4715 are amongst the brightest lines photographed with the prismatic camera during the total eclipse of the Sun on April 16, 1893.

The displacements of the bright lines described by Pickering are confirmed in the main by the Kensington photographs. In the first seven photographs in Plate I, taken between principal and secondary minimum, the bright lines lie on the less refrangible sides of the dark ones, at secondary minimum the broad bright lines are almost bisected by dark ones; while from secondary minimum to principal minimum the bright lines are more refrangible than the dark ones. The investigation of the movements of the bright lines must, however, be now carried on in the light of the knowledge gained, with regard to the existence of two sets of dark lines.

If we consider the displacements of the bright lines with reference to the dark lines of component R, we find that they are always in the same direction as those of component B with respect to R. Thus in the first half of the period, the bright lines, as well as the dark lines of component B, are less refrangible than those of component R, while during the second half they are more refrangible. The bright lines, however, do not keep a constant position with respect to those of component B, although displaced in the same direction.

8. *The bright lines are brightest soon after secondary minimum.*

If the brightness of the lines in reality remains constant, they will appear relatively brightest at the two minima, owing to the reduction of continuous spectrum which is associated with the increased brightness of the star at maximum, and for the same reason they should appear brighter at principal than at secondary minimum. Estimates of the brightness of the lines in relation to the continuous spectrum have been made independently by four of my assistants, and, although estimates of this kind are liable to error, the general agreement is sufficient to indicate that when all allowance is made for the varying continuous spectrum, there is a maximum of brightness of the bright lines about half a day after secondary minimum. The apparent increase of brightness near principal minimum seems to be due solely to the reduced intensity of the continuous spectrum.

I have to express my obligations to Messrs. Fowler, Baxandall, Shackleton, Butler, Wardale, Crabtree, and North, who, at different times, have assisted in taking the photographs.

ON BRESTER'S VIEWS AS TO THE TRANQUILITY OF THE SUN'S
ATMOSPHERE.*

EGON VON OPPOLZER.

Mr. A. Brester Jr., has recently published a totally new theory of the Sun. His fundamental statement is, that the atmosphere of the Sun is quite calm. Brester arrives at this conclusion by supposing that the different gases form layers, which are disposed according to their specific weight, and by assuming the absence of an atmospheric circulation resembling that produced by the different temperatures of pole and equator in the earth's atmosphere, which is the cause of its violent atmospheric motions. We shall see that the conclusions which he draws from these suppositions cannot be regarded as valid, as they are contradictory to established facts.

It is but natural to suppose that the gases in the atmosphere of the Sun should strive to range themselves according to their specific weight, as every movement tends to the restoration of equilibrium, which can only subsist with this disposition of layers. If, moreover, we take into consideration that the principal

* Communicated by the author.

movements in an atmosphere take place horizontally, it is evident, that the vertical disposition of layers can scarcely be subject to great disturbances. I have further shown in my paper "*Über die ursache der Sonnen flecken*" (*Sitzungsber. d. k. Ak. d. Wiss. Wien*) that an ascending or descending stream must ever under the most unfavorable suppositions become respectively 7° C. cooler, or hotter per km. change of vertical distance; hence in an ascent of 1'' = 721 km. a refrigeration, of 5000° C. must ensue. From this it is apparent that if the thickness of the strata be over 1'', a vertical movement can take place only with difficulty, and is in fact almost utterly checked. The expenditure of energy which would thereby be required, has been calculated by me in the same paper. Disturbances of the layers in a vertical sense, which would be shown by observation of the spectrum of the chromosphere can consequently scarcely be seen, the thickness of the disturbed, layer being much too insignificant. The opinion is forced upon us that the gases must be disposed in layers according to specific weight, notwithstanding even the most violent motions in the atmosphere.

In reference to the second point, that there is no general circulation in the Sun's atmosphere analogous to that in ours, the following remarks must be made. The atmospheric motions are caused by differences of temperature, which are, as known, transformed into differences of pressure and then into motion. It is generally acknowledged to be a mistake, that our atmosphere would be calm, if all latitudes should receive the same amount of warmth from the Sun

The difference in the warming of land and water produces such differences of pressure and such circulations, that the simple atmospheric circulation caused by the decreasing temperature in increasing latitude is relatively unimportant. Brester's theory leads necessarily to differences of temperature, and the simplest reasoning leads to the conclusion, that in the hot solar globe the temperatures are not merely a function of the distance from the solar centre; it would be positively absurd to entertain the idea, that the same temperature reigns in every part of the surface of the Sun. Cooler and hotter regions must exist. Consequently the existence of winds in the atmosphere of the Sun must be supposed *a priori*. Passing to the facts with which observations furnish us, we cannot doubt the existence of violent storms in the atmosphere of the Sun. The peculiar solar rotation alone, which is restricted to only an outer layer, confirms the opinion that winds must blow over the interior of the Sun, which turns like a solid

body. According to Dunér the daily angle of rotation on the equator amounts to $14^{\circ} 14'$ in a latitude 45° to 11.99° . This demonstrates the existence of winds blowing over the interior of the Sun with a rapidity of at least 100 meters persecond. To explain the peculiar rotation and apparently not to fall into contradiction with the assumption of a calm atmosphere, Brester propounds the hypothesis, that the flattened interior of the Sun is surrounded by a spherical atmosphere each having a different constant angle of rotation. This hypothesis is incompatible with mechanical principles, and moreover it would make a quite calm atmosphere impossible. If we consider the observation of sunspots, we find evidence of the most violent storms which last for days, and the average energy of which is more than three-fold as strong as that of the most violent storms of our atmosphere. The variation of spots in heliographical longitude and latitude demonstrates the existence of storms with a velocity of at least 100 metres. If we regard the first series of observations in Dunér's classical work. "*Recherches in la rotation du soleil*" (*Soc. Royal des 20 d' Upsal*), we find the following velocities per second at the equator:

1887 Date.		velocity (km.)
June	3.....	+ 2.25
"	3.....	+ 1.84
"	4.....	+ 1.79
"	4.....	+ 2.25

This again corresponds to differences of velocity of 460 metres, consequently winds of at least 230 metres must exist. That real importance must be attributed to these differences, is to be seen by the probable error of Dunér's observation, which amounts to 0.02 km. Even if we quote Brester's own words, we see that his own theory leads to the conclusion that violent storms must exist on the Sun's surface:

"My theory also explains the Moon's motions as shown by the spots. They owe their different angular velocities in different latitudes to the cloudy zones in which they are borne. If they frequently move a little more rapidly than these clouds it is because the gas, which in the growing spot pushes back the matter of the photosphere, must move particularly toward the side where the resistance is least. Now as this side is that of the foremost edge of the spot, every time a spot undergoes sudden changes it ordinarily advances on the solar surface by making a sort of leap.* This leap will also take place when, as the gaseous contents of a spot are recondensing, the vacuum thus produced will draw in

* Young, The Sun, p. 110.

again the surrounding photospheric matter. For this matter rushing in, preferably on the side where it already moves in the direction of motion, will fill up the spot from behind, once more giving to its center a sudden acceleration."*

Brester's theory is accordingly not based on sound reasoning; it assumes conditions *a priori* improbable and impossible, as for instance the state of repose, or the flattened interior of the Sun with spherical atmosphere turning with a different angular velocity, and it leads to consequences which are not consistent with the fundamental idea. Brester's theory like that of Schmidt can be ranged among the interesting ones,—interesting in that they upset all hitherto existing opinions, and still try to explain everything in an *apparently* natural way. As long as nothing prevents the acceptance of a very high temperature of the Sun, of a hardly conceivable evaporation of the gases above the photosphere, one is not justified in grasping at revolutionary ideas. A clear insight into the properties of the atmosphere, such as modern meteorology is leading us to, will alone help to a final solution of the phenomena about the surface of the Sun. Let us continue in the track beaten by Galileo and Kepler, who at once divined the meteorological nature of the sunspots.

WEIN DEN, 17 APRIL, 1894.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

The Spectrum of β Lyræ.—Since Pickering's discovery of the composite character of the spectrum of β Lyræ, spectroscopic literature relating to this star has been rapidly accumulating. Important papers on the photographic spectrum have been published by Belopolsky, Vogel, Sidgreaves and Lockyer, and in general the results contained in them are considered to be only preliminary, so that still more complete papers may be expected. While observers differ as to some of the details, they are in fair agreement as to the main features of the spectral changes, nor are the visual observations of the spectrum made at the Lick Observatory in disagreement with the photographic results, when one considers that they represent an imperfect view of phenomena which are more accurately and completely recorded by photography.

So far no satisfactory hypothesis has been framed which accounts for the complex spectral changes of β Lyræ. The assumption of two bodies of different

* ASTRONOMY AND ASTRO-PHYSICS, March 1894, p. 228.

character moving in a circular orbit satisfies the observations of the star's light period, but leaves unexplained or flatly contradicts some of the most striking changes in the spectrum. Professor Vogel considers that even the large mass of material accumulated at the Potsdam Observatory is insufficient to serve as the basis of a satisfactory explanation.

It is worthy of remark that none of the recent writers on the photographic spectrum make any reference to the anomalous variations of brightness of the D_3 and other bright lines, reported by earlier observers. These apparent variations were quite probably due to differences in atmospheric conditions and to the small aperture of the instruments employed. It is at least a suspicious circumstance that the D_3 line in the spectrum of γ Cassiopeiæ was observed at O'Gyalla in 1891 (Beobachtungen, Vol. XIII and XIV, p. 10) while during this year the star was frequently observed at Mt. Hamilton with the 36-inch equatorial and no line ever found at the place of D_3 .

The Appearance of the D_3 Line in the Spectrum of the Chromosphere.—In the May number of the *Memorie della Societa degli Spettroscopisti Italiani*, Herr Belopolsky gives some observations on the appearance of the helium line in the spectra of solar prominences. On a number of occasions a dark line was observed in the helium line, not in the middle, but somewhat toward one side. At first this was supposed to be a reversal of the line, and the unsymmetrical appearance was attributed to instrumental defects, but investigation showed that the appearance was real. A dark line was sometimes observed on the other side of the center.

As these absorption lines were not seen when the atmosphere was dry, but were quite distinct when it was moist, it was concluded that they are of telluric origin. The less refrangible line is double. The following measures of wavelengths (Potsdam system) were made:

5876.5 double
5876.0 D_3
5875.8

Researches on the Spectra of the Metals by Professor Hasselberg.—In vol. 26 of the Proceedings of the Royal Academy of Sciences of Sweden, Professor Hasselberg begins a series of monographs on the spectra of the metals. He points out in the introduction that until very recently our knowledge of the exact wavelengths of the lines of the metals was in a very unsatisfactory state, and quite inadequate to meet the demands of solar or even of stellar spectroscopy. The labors of Rowland and of Kayser and Runge have done much to remedy the difficulty, and it might even seem that such work as that of Hasselberg could now be regarded as superfluous. Aside from the value of independent confirmatory measurements, however, the different point of view from which Hasselberg regards the subject gives a special importance to his researches. The main object of Kayser and Runge was to discover numerical relations between lines in the spectra of the elements, and while it was important for their purpose that no line belonging to an element should be overlooked, it was of less consequence that lines belonging to some other element should be included. In Hasselberg's investigations special attention is paid to excluding all foreign lines.

The first of the series of monographs is on the spectrum of chromium. The wave-lengths are given on Rowland's scale, and are in close agreement with the results of Kayser and Runge. A carefully executed lithographed map accompanies the memoir.

The Photometric Catalogues of the Harvard College Observatory.—Professor Pickering replies, in A. N. 3229, to Dr. Chandler's criticisms of the Harvard photometric observations. The specific cases of error pointed out by Chandler are in general confirmed, but Professor Pickering objects to the inference that the magnitudes in the whole work are to be regarded with distrust. The errors referred to relate to variable stars, the observations of which are attended with unusual difficulties. "It is somewhat as though it should be argued from a physician's losing twenty per cent of his cholera patients that he had been equally unfortunate in his general practice." With the possible prejudice hinted at by Professor Pickering in this connection we have nothing to do; it is however natural to suppose that the attention of one specially interested in variable stars would be first attracted by errors relating to this class of objects.

With regard to the identification of stars observed with the photometer, Professor Pickering points out that when a star is observed out of the meridian the position of the reflecting mirror is recorded, together with the time, so that the approximate right ascension of the star can be determined from the record. The volume in which the observations of variable stars are to be discussed, and any errors in them considered has not yet been published.

For the great mass of stars in the Harvard Photometry which are not variable, comparison of the results with themselves and with other catalogues shows that the average error does not exceed one-tenth of a magnitude.

The Nature of Comet-Spectra.—In A. N. 3229, Professor Kayser replies to some comments by Professor Vogel on the paper translated in our May number, and gives some further consideration to Vogel's view that some comet-spectra are made up of the superposed spectra of carbon and carbon monoxide. It seems quite certain that if the CO bands were bright enough to cause a displacement in the maximum brightness of the principal carbon bands, such of them as fall at other parts of the spectrum would have been visible. Others would easily be detected by photography, which has recently been applied with so much success to the investigation of comet-spectra.

Leaving out of consideration the influence of the slit-width, the maximum brightness of the cometary bands should, according to Professor Vogel's explanation, always fall at certain definite wave-lengths. But the position of the maximum varies with the slit-width, and in fact very different positions have been found by different observers. As the slit-width used was not recorded, the observations are inconclusive, but the tables given by Professor Kayser will enable observers in the future to correct their measurements, and reduce them to the values that would have been obtained with a narrow slit.

It seems to us that, even with the advantage of the tables supplied by Professor Kayser, the results of visual observations would be subject to considerable doubt, except perhaps in the case of unusually bright comets. In this field, as in so many others, photography promises to be of the greatest usefulness.

New Observatories.—An editorial note in A. N. 3232 states that the founding of an Observatory at the University of Heidelberg is now an assured fact. The Observatory at Karlsruhe will be abandoned, and its instruments will be used in equipping the new Observatory, which will also have an astro-physical department. The site is on the Geisberg, about 270 metres above the level of the Rhine, and thirty-five minutes walk from the University.

The astronomical department will be under the direction of Professor Valentin; the astro-physical under that of Professor Max Wolf.

Dr. Lewis Swift has transferred his instruments including the 16-inch refractor from the Warner Observatory at Rochester to Echo Mountain, Los Angeles County, California. The new institution is called the Lowe Observatory. It is about 3,500 feet above the sea level and about two miles from the station on Wilson's Peak, formerly occupied by a party from Harvard College Observatory.

Radiation of Heated Gases.—In the *Philosophical Magazine* for March there is an article by Professor Smithells upon a subject which lies at the very foundation of astro-physics. Stated in terms of a concrete example, the question is why does a sodium bead turn the Bunsen flame yellow? Is the sodium chloride dissociated by heat, and does the free sodium atom then radiate light in virtue of its increased temperature?

Or, is there involved some process, perhaps like that of phosphorescence, which may be called, after E. Wiedemann, luminescence? Or is some purely chemical process, say reduction, an essential condition of luminosity? Or still again, is the phenomenon possibly an electrical one ultimately similar to that of the Geissler tube? Or is it true, as has been suggested, that in some, or all, cases we have two or more of these processes going on at the same time?

To state the question in *general* terms, are the conditions of Kirchhoff's law satisfied? If so is Kirchhoff's law itself satisfied? If not, what conditions are to be substituted?

The conditions of the above mentioned law are:

- (1) That the radiation emitted shall be at the expense of heat only, and
- (2) That the radiation absorbed shall assume the form of heat only.

The law in question states equality between the following two ratios, *viz*:

- (1) The ratio of the radiant energy of any one kind, emitted by a given substance at given temperature, to the radiation of the same kind given out by a perfectly black body at the same temperature, and
- (2) The ratio of the absorbed radiation to the incident radiation when the absorbing body is of the substance in question.

As ordinarily placed in symbols this reads:

$$\frac{E}{\epsilon} = A$$

Possibly it is simpler to say that $\frac{E}{A}$ is a constant for all bodies.

To this whole subject have recently been made a number of contributions which are of more than ordinary interest. Space forbids us to do much more than mention them.

Smithells' paper is preliminary. It contains a clear statement of the case, hints at some experimental difficulties, and concludes with an excellent critique of the work of Pringsheim.

To the latter is due the credit of having again started to rolling the ball which was first set in motion by Hittorf and W. Siemens. His (Pringsheim's) results may be found in *Wied. Ann.*, Bd. 45, p. 428, (1892), Bd. 49, p. 437 (1893), and Bd. 51, p. 441 (1894).

Briefly put, his views are that reduction processes are always at work in luminous flames, by which he means those flames which yield characteristic line spectra: that heat alone cannot bring out line spectra: and that, therefore, the conditions of Kirchhoff's laws are not satisfied, at least in luminous flames.

Still more recently, Paschen has taken up the subject and has pursued it with

great skill. His work is contained in three papers, the first of which was reviewed in this journal two months ago; the other two will be found in the current volume of *Wiedemann's Annalen* (Bd. 51, pp. 1-39 and pp. 40-46)

Paschen notes ambiguity in Pringsheim's use of the expression "characteristic spectrum," and proceeds to map and compare the absorption and emission spectra of carbon dioxide and water vapor, between the temperatures of 100° C. and 500° C. Kirchhoff's law he finds for these gases is certainly true in a qualitative, and probably true in a quantitative, way. His view is that this law is satisfied, both as to condition and content, in many radiating bodies, but that, in luminous flames, more is involved than mere heating; indeed, in all cases in which the radiation exceeds that of a black body at the same temperature, luminescence of some kind is at work.

The chief value of Pringsheim's work is perhaps that it confirms the view that a *uniform* temperature, equal to or less than that of the blast furnace, is incompetent to bring out the bright line spectra of metallic vapors. Concerning what may happen at higher temperatures no inference is to be drawn from his experiments.

Paschen, on the other hand, shows that since certain heated gases *do* obey Kirchhoff's law, they *probably* satisfy its conditions, *i. e.*, their radiation is probably due to heat alone. His papers are full of suggestion at every point; and the outline given above is extremely meagre.

Taken all together, these experiments show that the problem of the Bunsen burner is surprisingly complicated; and, so far from being solved, is as yet barely capable of clear statement.

We have here also fair warning against hasty interpretation of stellar and solar spectra. Why is one line sharp and another hazy? What is the meaning of an asymmetric reversal? Is not motion in the line of sight sometimes a great convenience? Is not luminescence a happy word to cover our ignorance? H. C.

Solar Observations at the Royal Roman College.—The following is an extract from a letter from Professor Tacchini:

I send you a resumé of the solar observations made during the first quarter of the year 1894.

SPOTS AND FACULÆ.

1894.	No. of days of Observations.	Relative Frequency.		Relative Size.		No. of Groups. per day.
		of Spots.	of days with- out spots.	of Spots.	of Faculæ.	
January	19	24.37	0.00	106.1	74.2	7.2
February	20	19.35	0.00	136.3	65.8	6.3
March	20	17.51	0.00	48.1	57.5	4.8

PROTUBERANCES.

1894.	Average No. per Day.	Average Height per Day.	Average Breadth per Day.
January	14	6.00	1.6
February	18	7.17	2.6
March	18	8.11	2.2

For the spots and faculæ a progressive diminution is shown. The great extent of the area of spots in February is due to the great spot in the southern hemisphere (-24° to -35°). The phenomena of the protuberances, on the other hand show an increase when compared with the last quarter of 1893.

For the distribution of solar phenomena according to the latitude, we have obtained the following results:

FIRST QUARTER OF YEAR 1894.

Latitude.	Protuberances.	Faculae.	Spots.
90 + 80	0.000		
80 + 70	0.000		
70 + 60	0.003		
60 + 50	0.018		
50 + 40	0.008	0.000	
40 + 30	0.039	0.005	0.000
30 + 20	0.080	0.072	0.062
20 + 10	0.088	0.154	0.185
10 + 0	0.088	0.197	0.144
			0.391
0 - 10	0.057	0.197	0.155
10 - 20	0.065	0.192	0.330
20 - 30	0.111	0.120	0.103
30 - 40	0.103	0.048	0.021
40 - 50	0.013	0.010	
50 - 60	0.015		
60 - 70	0.222		
70 - 80	0.080		
80 - 90	0.010		

All the solar phenomena were most frequent in the southern zones, and the same result is found for each month of the quarter. An unusual maximum of protuberances is to be noticed in the zone -60° to -70° which is also found for each month, while protuberances are very infrequent between $+40^{\circ}$ and $+70^{\circ}$, and fail entirely between $+70^{\circ}$ and $+90^{\circ}$. We have on only one occasion, on March 1, found evidences of eruption in latitude -15° .

P. TACCHINI.

CURRENT CELESTIAL PHENOMENA.

Planet Notes for September and October.

Mercury will be at superior conjunction Sept. 2 and will be in poor position for observation during the two months. He will be in conjunction with Saturn Sept. 30 and with Uranus Oct. 14. He will be at greatest eastern elongation, $24^{\circ} 31'$ E. from the Sun, on the morning of Oct. 19. In the evening about this time Mercury, to northern observers, will set only a half hour after the Sun, so that it can be seen only in bright twilight. In the southern hemisphere the conditions for observation will be better.

Venus will remain "morning star" during these months, steadily approaching the Sun and growing fainter. She will be in conjunction with the Moon Sept. 27 and Oct. 27. On Oct. 9 at $10^h 56^m$ A. M. Venus will be just $7'$ north of the star η Virginis, and on Oct. 29 at $10^h 07^m$ A. M. she will be $1^{\circ} 06'$ south of Saturn. Both however will be too close to the Sun to be easily seen.

Mars during these months will be in excellent position for observation. He will be at opposition Oct. 20. His distance from the Earth will then be about 40,500,000 miles, or about 5,000,000 miles greater than it was at the opposition of 1892. His declination, however, is 33° further north, so that for northern observers the planet is in very much better position than in 1892. Professor Pickering has already reported interesting observations of the surface markings of the planet, made at the new Lowell Observatory at Flagstaff, Arizona, and it is not too much to expect that more and better observations will be obtained this

year than ever before. Mars is now in the constellation Pisces moving eastward. Sept. 15 he will turn the loop in his apparent course and begin retrograde (westward) motion, remaining in Aries and the corner of Pisces during the two months. The reader will easily recognize Mars by the ruddy color and great brilliancy, this being the brightest object in the southeastern sky. Mars will be 7° south of the Moon Sept. 18 at $10^{\text{h}} 49^{\text{m}}$ A. M. and $5^{\circ} 31'$ south of the same on Oct. 15 at $6^{\text{h}} 31^{\text{m}}$ A. M.

Jupiter is the brilliant star one sees rising a little to the north of east soon after midnight. In October Jupiter will be in position to be observed a little before midnight. He will be at quadrature, 90° east from the Sun Sept. 28; at conjunction with the Moon Sept. 22 at $3^{\text{h}} 09^{\text{m}}$ and Oct. 19 $11^{\text{h}} 05^{\text{m}}$ P. M. Jupiter is in the feet of Gemini moving eastward, but will begin retrograde movement Oct. 24.

Saturn and *Uranus* will not be in position for observation, Saturn reaching conjunction with the Sun Oct. 21 and Uranus Nov. 7.

Neptune may be observed after midnight. He is in Taurus quite near the sixth magnitude star ι Tauri.

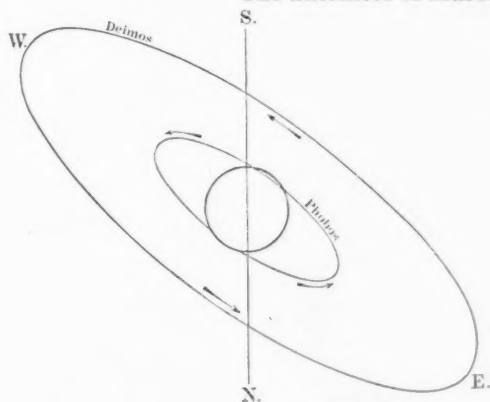
Planet Tables for September and October.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY.						
Date.	R. A. h m	Decl. °	Rises. h m	Transits. h m	Sets. h m	
Sept. 5.....	11 08.8	+ 7 12	5 38 A. M.	12 09.4 P. M.	6 41 P. M.	
15.....	12 13.1	- 0 39	6 33 "	12 34.3 "	6 35 "	
25.....	13 10.7	- 8 01	7 21 "	12 52.4 "	6 24 "	
Oct. 5.....	14 04.2	- 14 23	8 01 "	1 06.4 "	6 12 "	
15.....	14 53.0	- 19 20	8 57 "	1 15.7 "	5 59 "	
25.....	15 29.6	- 22 05	8 43 "	1 12.9 "	5 43 "	
VENUS.						
Sept. 5.....	9 35.6	+ 15 16	3 31 A. M.	10 36.5 A. M.	5 42 P. M.	
15.....	10 23.5	+ 11 19	3 56 "	10 45.0 "	5 34 "	
25.....	11 10.3	+ 6 50	4 22 "	10 52.2 "	5 23 "	
Oct. 5.....	11 56.2	+ 2 01	4 48 "	10 58.9 "	5 10 "	
15.....	12 42.0	- 2 57	5 13 "	11 05.1 "	4 57 "	
25.....	13 28.3	- 7 51	5 40 "	11 12.0 "	4 44 "	
MARS.						
Sept. 5.....	2 13.6	+ 9 29	8 30 P. M.	3 11.8 A. M.	9 53 A. M.	
15.....	2 16.0	+ 9 48	7 52 "	2 34.9 "	9 18 "	
25.....	2 12.7	+ 9 45	7 10 "	1 52.3 "	8 35 "	
Oct. 5.....	2 03.9	+ 9 23	6 23 "	1 04.3 "	7 45 "	
15.....	1 51.6	+ 8 49	5 34 "	12 12.7 "	6 51 "	
25.....	1 38.4	+ 8 15	4 44 "	11 20.1 P. M.	5 56 "	
JUPITER.						
Sept. 5.....	6 12.2	+ 23 03	11 27 P. M.	7 09.8 A. M.	2 53 P. M.	
15.....	6 17.7	+ 23 02	10 53 "	6 35.8 "	2 19 "	
25.....	6 22.0	+ 23 00	10 18 "	6 00.9 "	1 43 "	
Oct. 5.....	6 25.2	+ 22 59	9 42 "	5 24.7 "	1 07 "	
15.....	6 27.1	+ 22 58	9 05 "	4 47.2 "	12 30 "	
25.....	6 27.5	+ 22 59	8 26 "	4 08.4 "	11 51 "	
SATURN.						
Sept. 5.....	13 27.8	- 6 43	8 51 A. M.	2 28.0 P. M.	8 05 P. M.	
15.....	13 31.6	- 7 07	8 17 "	1 52.6 "	7 28 "	
25.....	13 35.8	- 7 33	7 44 "	1 17.5 "	6 51 "	
Oct. 5.....	13 40.2	- 7 59	7 11 "	12 42.6 "	6 14 "	
15.....	13 44.8	- 8 26	6 38 "	12 07.7 "	5 38 "	
25.....	13 49.3	- 8 52	6 05 "	11 32.9 "	5 01 "	

URANUS.						
Date.	R. A.	Decl.	Rises.	Transits.	Sets.	
1894.	h m	°	h m	h m	h m	
Sept. 5	14 39.5	- 15 10	10 38 A. M.	3 39.7 P. M.	8 42 P. M.	
15	14 41.2	- 15 18	10 01 "	3 02.0 "	8 03 "	
25	14 43.1	- 15 27	9 24 "	2 24.7 "	7 25 "	
Oct. 5	14 45.2	- 15 37	8 47 "	1 47.3 "	6 47 "	
15	14 47.4	- 15 47	8 11 "	1 10.3 "	6 09 "	
25	14 49.8	- 15 58	7 35 "	12 33.3 "	5 32 "	
NEPTUNE.						
Sept. 5	4 58.9	+ 21 13	10 23 P. M.	5 56.6 A. M.	1 30 P. M.	
15	4 59.0	+ 21 13	9 44 "	5 17.4 "	12 51 "	
25	4 59.0	+ 21 12	9 05 "	4 38.0 "	12 11 "	
Oct. 5	4 58.7	+ 21 11	8 25 "	3 58.4 "	11 32 "	
15	4 58.2	+ 21 10	7 45 "	3 18.6 "	10 52 "	
25	4 57.4	+ 21 09	7 05 "	2 38.6 "	10 12 "	
THE SUN.						
Sept. 5	10 57.9	+ 6 38	5 28 A. M.	11 58.5 A. M.	6 29 P. M.	
15	11 33.8	+ 2 50	5 39 "	11 55.0 "	6 11 "	
25	12 09.7	- 1 03	5 51 "	11 51.5 "	5 52 "	
Oct. 5	12 46.0	- 4 56	6 03 "	11 48.3 "	5 33 "	
15	13 22.8	- 8 43	6 16 "	11 45.8 "	5 16 "	
25	14 00.6	- 12 17	6 29 "	11 44.1 "	4 59 "	

The Satellites of Mars.



DEIMOS.

Sept. 24	8.3 A. M.	W.
26	5.7 "	E.
28	3.1 "	W.
30	12.5 A. M.	E.
Oct. 1	9.9 P. M.	W.
3	7.3 "	E.
5	4.7 "	W.
7	2.1 "	E.
9	11.5 A. M.	W.
11	9.0 "	E.
13	6.5 "	W.
15	3.8 "	E.
17	1.2 "	W.
18	10.6 P. M.	E.
20	8.0 "	W.
22	5.5 "	E.
24	2.9 "	W.
26	12.3 "	E.
28	9.7 A. M.	W.
30	7.1 "	E.

PHOBOS.

Sept.	h		Oct.	h		Oct.	h	
24	4.6 A. M.	W.	6	11.2 "	E.	18	5.8 "	W.
25	7.4 "	E.	7	2.0 P. M.	W.	19	8.6 "	E.
26	10.2 "	W.	8	4.8 "	E.	20	11.4 "	W.
27	1.0 P. M.	E.	9	7.6 "	W.	22	2.2 A. M.	E.
28	3.7 "	W.	10	10.4 "	E.	23	5.0 "	W.
29	6.5 "	E.	12	1.1 A. M.	W.	24	7.7 "	E.
30	9.3 "	W.	13	3.9 "	E.	25	10.5 "	W.
Oct. 2	12.1 A. M.	E.	14	6.7 "	W.	26	1.3 P. M.	E.
3	2.9 "	W.	15	9.5 A. M.	E.	27	4.1 "	W.
4	5.7 "	E.	16	12.3 P. M.	W.	28	6.9 "	E.
5	8.4 "	W.	17	3.0 "	E.	29	9.7 "	W.
						31	12.5 A. M.	E.

For Phobos the central time of every seventh eastern and western elongation is given, and for Deimos every third; the intermediate ones may be found by adding the periodic time of each satellite. Periodic time of Phobos $7^h 39^m.2$. Periodic time of Deimos $1^d 6^h 17^m.9$

Phases and Aspects of the Moon.

		<i>Central Time.</i>		
		d	h	m
First Quarter.....	Sept.	6	7	03 P. M.
Apogee.....		10	2	12 A. M.
Full Moon.....		14	10	21 P. M.
Last Quarter.....		22	6	32 A. M.
Perigee.....		25	11	30 P. M.
New Moon.....		28	11	44 "
First Quarter.....	Oct.	6	1	01 "
Apogee.....		7	6	48 "
Full Moon.....		14	12	41 "
Last Quarter.....		21	12	56 "
Perigee.....		22	7	42 A. M.
New Moon.....		28	11	57 "

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.			λ TAURI CONT.			U OPHIUCHI.		
Alternate Minima.			h			Every fourth Minimum.		
		h	Oct.	5	1 A. M.	Sept.	3	10 A. M.
Sept.	3	6 P. M.		12	11 P. M.		6	7 P. M.
	8	5 "		20	8 "		10	3 A. M.
	13	5 "		28	6 "		13	noon
	18	5 "	R. CANIS MAJORIS.				16	8 P. M.
	23	4 "	Every third minimum.				20	5 A. M.
	28	4 "	Sept.	3	2 A. M.		23	1 P. M.
Oct.	3	4 "		6	noon.		26	10 P. M.
	8	3 "		9	9 P. M.		30	6 A. M.
	13	3 "		13	7 A. M.	Oct.	3	3 P. M.
	18	3 "		16	5 P. M.		6	11 "
	23	2 "		20	3 A. M.		10	8 A. M.
	28	2 "		23	1 P. M.		13	4 P. M.
ALGOL.				26	10 "		17	1 A. M.
				30	8 A. M.		20	9 "
Alternate Minima.			Oct.	3	6 P. M.		23	6 P. M.
Sept.	5	4 P. M.		7	4 A. M.		27	2 A. M.
	13	10 A. M.		10	2 P. M.		30	11 "
	17	4 "		13	11 "	Y CYGNI.		
	22	9 P. M.		17	9 A. M.	Every fourth minimum.		
	28	3 "		20	7 P. M.	Sept.	4	9 A. M.
Oct.	4	8 A. M.		24	5 A. M.		10	9 "
	10	2 "		27	2 P. M.		16	9 "
	15	8 P. M.		30	midn.		22	8 "
	21	1 "	S. CANCRI.				28	8 "
	27	7 A. M.	Sept.	2	6 P. M.	Oct.	4	8 "
λ TAURI.				12	6 A. M.		10	8 "
Alternate Minima.				21	6 P. M.		16	8 "
Sept.	2	10 A. M.	Oct.	1	5 A. M.		22	8 "
	11	8 "		10	5 P. M.		28	7 "
	19	5 "		20	5 A. M.			
	27	3 "		29	4 P. M.			

Occultations Visible at Washington.

Date 1894	Star's Name.	Magni- tude.	IMMERSION			EMERSION			Duration.
			Washing- ton	Angle M. T. f'm	N p't.	Washing- ton	Angle M. T. f'm	N p't.	
			h m	°		h m	°		
Sept. 11	ϕ Capricorni.....	5½	5 48	12		6 27	311		0 39
13	70 Aquarii.....	6	6 21	19		7 09	288		0 48
13	Lalande 44734.....	7	9 04	46		10 25	239		1 21
18	47 Arietis.....	6	16 36	65		17 56	247		1 20
19	9 Tauri.....	7	8 02	10		8 27	307		0 25
19	23 Tauri.....	5	11 44	58		12 51	247		1 07
19	24 Tauri.....	8	12 19	48		13 27	256		1 08
19	η Tauri.....	3	12 21	53		13 31	251		1 10
19	B. A. C. 1171.....	8	12 59	28		14 10	276		1 11
19	27 Tauri.....	4	13 10	105		14 08	200		0 58
19	28 Tauri.....	6	13 10	86		14 20	219		1 10
23	ω^1 Cancri.....	6	14 05	13		14 13	358		0 08
23	ω^2 Cancri.....	6	14 02	98		15 02	273		1 00
25	26 Leonis.....	8	14 03	148		14 42	247		0 39
Oct. 6	B. A. C. 6628.....	6	10 23	121		11 04	197		0 41
	χ Capricorni.....	5½	12 21	117		12 55	184		0 34
11	B. A. C. 8184.....	6	15 20	42		16 23	257		1 03
13	B. A. C. 221.....	6	6 24	64		7 29	227		1 05
13	70 Piscium.....	8	16 35	54		17 34	254		0 59
13	ϵ Piscium.....	4	16 53	83		17 48	227		0 55
15	27 Arietis.....	6	9 03	47		10 09	245		1 06
16	B. A. C. 1055.....	7	8 20	101		9 06	204		0 46
16	66 Arietis.....	5	10 28	42		11 34	258		1 06
17	χ Tauri.....	6	8 06	78		8 56	232		0 50
19	49 Aurigæ.....	6	10 37	73		11 34	275		0 57
19	54 Aurigæ.....	6	12 36	35		13 20	315		0 44
19	25 Geminorum.....	6	13 16	61		14 22	293		1 06
20	ϵ Geminorum.....	6	13 10	146		13 51	223		0 41

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Gesellschaft".]

MAXIMA.		MAXIMA CONT.		MINIMA CONT.	
Sept. 1	X Libræ.	Oct. 8	S Camelopardi.	Sept. 20	S Libræ.
2	T Arietis.	12	T Virginis.	20	R Scuti.
5	U Piscium.	13	S Aquilæ.	20	R Canis Min.
6	T Geminorum.	13	R Arietis.	22	R Trianguli.
7*	T Delphini.	14	V Leonis.	23	α Ceti.
8	R Centauri.	14	W Leonis.	29	R Sculptoris.
13	V Aurigæ.	16	W Herculis.	29	W Cygni.
14	Y Capricorni.	17	W Tauri.	Oct. 1	RT Cygni.
16	U Monocerotis.	20	R Serpentinis.	2	R Piscium.
16	T Aquarii.	21	W Capricorni.	6	U Boötis.
18	U Canis Min.	23	X Boötis.	10	X Capricorni.
18†	V Capricorni.	23	RR Virginis.	10	R Sagittæ.
19	R Lyræ.	25	R Scuti.	13	U Monocerotis.
20	S Leonis.	26	R Sagittæ.	20	R Lyræ.
29	S Carini.	26	S Geminorum.	22	R Vulpeculæ.
29	R Draconis.	26	U Virginis.	22	R Persæ.
30	R Canum Ven.	28	X Scorpii.	24	S Boötis.
30	R Camelopardi.	31	U Monocerotis.	24	R Andromedæ.
Oct. 1	S Vulpeculæ.			26	U Boötis.
4	W Libræ.			27	R Virginis.
4	R Orionis.	Sept. 4	R Lyræ.	30	Z Cygni.
4	S Orionis.				
5	R Reticuli.	5	U Orionis.		

* The "Companion to the Observatory" gives this as Sept. 9.

† The "Companion to the Observatory" gives this as Sept. 22.

A Partial Eclipse of the Moon will occur on the night of Sept. 14, 1894. It will be visible throughout North and South America. The beginning will be visible in the western part of Europe and Africa. The accompanying diagram will give the reader some idea of the Moon's course as it passes by the Earth's shadow. The large shaded circle represents a cross-section of the Earth's shadow and the small circles represent the Moon at first and last contacts and middle of eclipse. The Moon will pass by the lower edge of the shadow, touching it first at the southernmost point. The observer will therefore see the shadow first at the north point of the Moon's disk. As the Moon moves up toward the left the

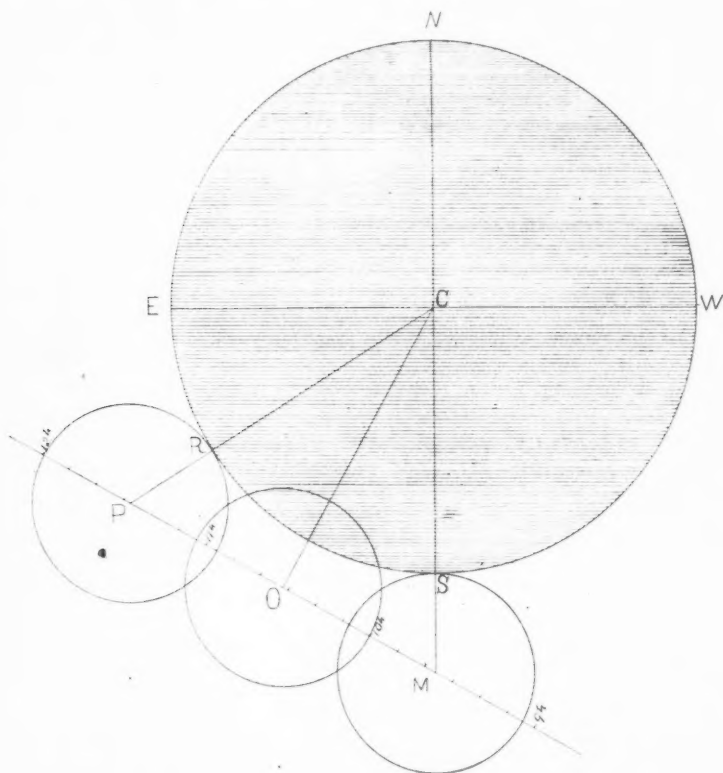


DIAGRAM SHOWING THE COURSE OF THE MOON BY THE EARTH'S SHADOW, DURING THE PARTIAL ECLIPSE, SEPT. 14, 1894.

shadow will appear to move down toward the right, covering at the middle of the eclipse a little less than a quarter of the diameter of the Moon's disk, and leaving it at a point 58° to the west from the north point. The first contact will occur at $9^{\text{h}} 36^{\text{m}}$, central standard time, the Moon's center being then at the point M. Before this a faint shading, due to the penumbra of the Earth's shadow,

will have been noticed on the upper part of the disk. At $10^h 32^m$ the Moon will be at O, and the eclipse at its maximum. At $11^h 28^m$ the Moon will be at P leaving the shadow at R. After that there will be only the faint penumbral shading on the west side of the disk.

ELEMENTS OF THE ECLIPSE.

Greenwich mean time of conjunction in right ascension Sept. 14, $15^h 35^m 42^s.8$.

Sun's right ascension.....	$11^h 31^m 36^s.20$	Hourly motion.....	$8^s.97$
Moon's right ascension...	$23 31 36.20$	Hourly motion.....	109.98
Sun's declination.....	$3^\circ 04' 10''.0$ N	Hourly motion.....	$0' 57''.8$ S
Moon's declination.....	$3 59 33.5$ S	Hourly motion.....	$14 52.6$ N
Sun's equa. hor. parallax	8.5	Sun's true semi-diameter	$15 54.9$
Moon's equa. hor. "	$55 24.1$	Moon's " " "	$15 05.6$

TIMES OF THE PHASES.

	h	m	
Moon enters penumbra.....	Sept. 14	7 58.6	P. M. }
Moon enters shadow.....		9 35.6	" }
Middle of eclipse.....		10 31.6	" }
Moon leaves shadow.....		11 27.7	" }
Moon leaves penumbra.....	Sept. 15	1 04.4	A. M. }

Central Standard Time.

A Total Eclipse of the Sun will occur Sept. 28, 1894. It will be invisible in America. The path of totality passes across the Indian Ocean as shown in the accompanying cut. The eclipse will be partial in Africa, Persia, Hindostan and

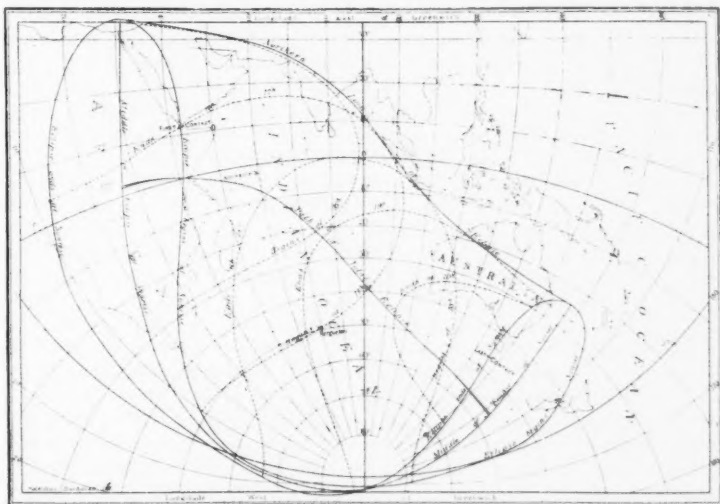


CHART OF THE TOTAL ECLIPSE OF THE SUN, SEPT. 28, 1894.

southern Australia. The times marked on the chart are expressed in Greenwich mean time.

CIRCUMSTANCES OF THE ECLIPSE.

	Greenwich M. T.			Longitude from Greenwich.		Latitude.	
		h	m	°	'	°	'
Eclipse begins.....	Sept. 28	15	01.0	42	50.7 E.	11	49.2 N.
Central eclipse begins...		16	03.7	26	44.3 E.	1	47.1 N.
Eclipse at noon.....		18	06.2	86	01.3 E.	34	11.6 S.
Central eclipse ends.....		19	14.1	162	43.3 E.	56	24.9 S.
Eclipse ends.....		20	17.0	145	54.0 E.	46	24.1 S.

NEWS AND NOTES.

It is our custom to send not more than one number of this magazine after subscriptions have expired unless notified by subscribers that the publication should be continued. Usually we have informed our patrons by letter of the time when renewals should be made.

Hereafter it is the intention of the publisher to mail this periodical on or about the 25th of the month preceding the one for which it is dated instead of the last day of the month as heretofore. This change is made to accommodate foreign subscribers whose patronage is increasingly large, and whose wishes are therefore correspondingly important. Contributors are respectfully asked to bear this in mind, and to send in articles not later than the 12th and minor notices on or before the 18th of the month of issue.

Professor George E. Hale writes from Palermo under date of June 20 as follows:

"I am delayed here on account of snow on Mt. Etna which has hitherto prevented the pack animals from reaching the Observatory. Within a week, however, we hope to make the ascent, though we may be still further delayed by some apparatus in transit from Germany. We have been in southern Italy and Sicily about three weeks, but during the whole time we have not seen the sky as blue as it frequently is in Chicago. The season is said to be a very exceptional one, and I much fear that even the altitude of Etna will not be sufficient to take us out of the mist which seems to enshroud everything."

It will be remembered, from previous notices, that one object of Professor Hale's visit to Etna at this time is to study the Sun's corona without an eclipse by the aid of photography if possible.

C. M. Charroppin, S. J., writes us from Corozal, British Honduras, under date of June 20, that "Mars is now in splendid position, being high in the heavens; but, my telescope is entirely too small for useful observations. I am at present negotiating with the states for a six-inch. I hope to get it before the time of the transit of Mercury. I was very much amused to receive a _____ paper (from the states) with the following notice: 'Fr. Charroppin has gone to British Honduras to observe the transit of Venus.' He will be the only astronomer in that island to observe this rare phenomenon.' It is pardonable for an editor to call the transit of Mercury the transit of Venus; but to call British Honduras an island is a blunder that ought to make the paper a back number."

Scheiner's Spectral Analyse der Gestirne.—Early in June the publishers (Messrs.

Ginn & Co.) of Frost's translation of Scheiner's *Spectral Analyse der Gestirne* announced that more time was needed for the publication of the book than was anticipated, but it was then thought that it would be issued before the end of June. The work has not yet appeared so far as we know, but doubtless it will soon be ready for the market. We have not the least doubt but that the book will be benefited by the delay, for such publications cannot be hurried very safely.

Auroræ and Solar Prominences.—Upon the night of June 6th, when the eruptive prominences which are described as having been seen on June 7th at Goodsell Observatory, were exactly on the Sun's limb there was an aurora with streamers. Upon June 9th there was another prominent aurora—another section of this same area of disturbance being at the limb at that time also. These displays were best seen apparently in Canada. Further details will be had when reports for the month are at hand.

It is of interest to note that June 7th was the 17th day of the synodic period as I have them arranged, this being the date of a long and very prominent series of auroras and their clusters, which has been the most persistent of any now in progress at the 27 $\frac{1}{4}$ day interval.

M. A. VEEDER.

The Pulkova Catalogue for 1885.—They who have read the reports and other publications of the Pulkova Observatory are aware that, besides the standard catalogue for 1845 mentioned in Professor Eastman's address, there is also one for 1865 which is the basis of the present Jahrbuch ephemerides. This is far more accurate than the former; in right ascension because the chronographic method was added to the eye-and-ear method; in declination because of more complete observations according to the programme. A later one for 1885 is now in progress and the declinations are published.

They are still more completely observed and discussed. The instrument is the same as before; a vertical circle. But it has been re-divided by the Repsolds. The same method is used; that is every observation is double; the instrument was rotated on its vertical axis after taking one altitude, and another taken in reversed position. The level is of course used to indicate the zero-point.

The variation of the latitude is observed and allowed; the refractions are re-discussed allowing for the difference between exterior and interior temperatures.

The final result is to indicate that the declinations for 1865 brought up with the proper motions from Auwers and Bradley are very nearly in accordance with those for 1885, the average difference being less than 0.05. The new Pulkova declinations then are a most important and welcome confirmation of those given in the Berlin Jahrbuch.

T. W. SAFFORD.

Williamston, July 5, 1894.

A Device for Securing a Mercury Surface Undisturbed by Earth Tremors.—In making observations by reflection from a mercury horizon, the observer is frequently troubled on account of agitation of the mercury. This is especially troublesome in the neighborhood of a city or railroad. On the pier of the transit circle of the University of Minnesota the disturbance is so great and so constant, that it is impossible to see the reflected images of the wires until late at night. Even then they are seen with great indistinctness. To overcome this difficulty Mr. Saegmuller sent with the transit circle a bunch of felt cloth and a copper disk. His instructions were to put the felt cloth in the mercury cup, place the

disk upon it and fill the slightly concave surface of the disk with mercury. A trial of this method showed the images to be somewhat improved, but they were still seen with great difficulty. However, when the felt cloth was removed and the disk floated on the mercury, the reflecting surface upon the disk was found to be absolutely quiet. The images of the wires came out with perfect distinctness at any time and under any ordinary condition of disturbance. During the passing of a railroad train, several hundred yards distant, a slight oscillatory motion of the images was noticed.

The success of this method is due to two causes. First since the depth of mercury on the disk is slight, the attraction of the copper tends to prevent surface waves. Second because the disk floats in mercury, the outside surface waves merely break upon the sides of the disk without imparting their motion to it.

F. P. LEAVENWORTH.

University of Minnesota, July 9, 1894.

Nova Aurigæ.—On the mornings of July 12 and 14 I examined this object with the 12-inch.

It has not changed in brightness from the last observations in the spring.

It is apparently the slightest bit brighter than the star which Mr. Burnham has called F in his early measures of the Nova, and which is in R. A. 32° and distance $85''$.

E. E. BARNARD.

Mt. Hamilton, July 14, 1894.

Temple's Periodic Comet.—This faint comet is now being observed with the 12 inch equatorial. It is extremely faint with that instrument. About $\frac{1}{2}'$ in diameter with scarcely any sensible brightening in the middle.

It promises to be observable for some time yet.

E. E. BARNARD.

Mt. Hamilton, July 14, 1894.

In my paper on the Proper Motion of the Stars in the Dumb-Bell Nebula, *ASTRONOMY AND ASTRO-PHYSICS* for June, I desire to make the following correction on p. 447.

For the star d in the $\Delta\delta$ for

	+ 15".79
	+ 16 .27

	+ 16".37
read	+ 15".84
	+ 16 .37

	+ 16".10

This gives for the comparison with Struve in $\Delta\delta$ for d

		Diff. from Struve.
Struve	+ 12".8	
Wilson	+ 16 .6	- 3".8
Barnard	+ 16 .1	- 3. 3

Differing from the value in June A. AND A.-P. by $0''.3$

E. E. BARNARD.

Photometric Catalogues of the Harvard College Observatory.—In *Astronomische Nachrichten*, Vol. 134, p. 355, S. C. Chandler severely criticises the photometric observations which have been published in some of the catalogues of Harvard

College Observatory. E. C. Pickering, the director of the above named Observatory answers these criticisms in *A. N.*, Vol. 135, p. 220. In the article referred to Mr. Chandler claims that upon examination, "It soon became manifest that there were numerous incongruities in the observations given" in Vol. XXIV of the Harvard College Observatory Annals, so much so, as to leave upon him, "an impression of distrust whether any of these observations are suitable for any precise or critical purpose." Then follows fifteen citations of error in support of this lively distrust, all of which are variable stars, ranging at their minima, from the 9th to the 14th magnitude in brightness. The hypothesis which Mr. Chandler assumes as reasonable for the cause of these errors, is generally the observation of wrong stars.

He next says, "still stronger presumption exists of a similar defect in the observations for the Photometric Catalogue of bright stars in Vol. XIV, where the discordances are yet more startling and numerous and have puzzled astronomers who have had occasion to examine those results critically." In support of this sweeping "presumption" three observations of one star are given, and he adds in the same paragraph that "this object is a faint telescopic star of 9.5 magnitude which must have crept into the the working-list from failure to notice the correction BB. VI, p. 378. But to go into the errors in the Photometric Catalogue lies outside of the intended scope of this note."

Director Pickering's answer to this attack is explicit, courteous and dignified. He says the only error pointed out in Vol. XIV had been detected at Harvard Observatory, and an explanation forwarded months ago to the superintendent of the British Nautical Almanac. These observations were among the first undertaken, and the errors were due to errors in other catalogues.

Fifteen instances of error are also pointed out in a total list of eighty-six variable stars whose observations are printed in Vol. XXIV, and Mr. Chandler's assumption is that similar errors exist throughout the entire Catalogue of over 20,000 stars contained in the same volume. Of these fifteen stars Professor Pickering says the original records show that eight places were wrong because of wrong reduction originally, and that the plan of observation afforded a means of satisfactory identification of the right stars in each case. He points out three errors that Mr. Chandler made in trying to locate the stars he observed and to give the right magnitude. Two other errors Professor Pickering says were made with the large meridian photometer when it was first employed. The original record of the observation of another star observed in 1888 is missing. In the case of two others the mis-identified stars are so close to the positions of the stars named that the magnifying power used on the photometer would not make the difference in position perceptible.

Director Pickering also claims that it can not be assumed that the observations of variable stars of long period are wrong because their measured magnitudes do not agree with those found by prediction. For α Ceti has been observed more than four centuries, yet its time for maximum, this year, as predicted by Mr. Chandler was Feb. 17, but the maximum really came more than one month later than the prediction. It is also a well known fact that the different maxima of this star vary by as much as three whole magnitudes. Criticisms on the errors of magnitudes in long period variable stars is not very safe business in the present state of progress of variable star astronomy. In the light of Professor Pickering's answer it seems to us that Mr. Chandler has greatly exaggerated his case whether he is conscious of it or not (think he is not), he has unjustly and wrongfully berated reasonably good work. Such an arraignment is not a high-minded benefaction to science. It can not be esteemed an enviable honor for any

of its truth-loving votaries to wear. We do not wonder that Dr. Gould should feelingly disclaim any share in the onus of such a thing in the minds of people generally interested. On the other hand how different are the newspaper mouthings of a few science fledglings at the "Hub" who pose as judges supreme on the merits or demerits of "Old Harvard." Judged by the true scientific spirit the two views are as far apart as the east is from the west.

The Radiant of the August Perseids.—Mr. Monck's repeated attacks on the motion of the Perseid radiant point remind me forcibly of the late John Hampden's assaults on the rotundity of the Earth. The latter gentleman, it is true, never succeeded in making the globe flat, for it apparently still retains its spherical shape, nor will all Mr. Monck's endeavors alter the behavior of the Perseid radiant by bringing it to a standstill, for in the ordinary routine of Nature it will go on moving much the same as at present until the end of time!

This motion is amply proved both by observation and theory. Mr. Monck avers there is no motion though he has never investigated the feature either in its observational or mathematical bearings. Being unacquainted with either the one or the other he is obviously not qualified to speak upon the matter at all, for, to be perfectly frank with him, he cannot know what he is talking about. He is trying to raise a superstructure without putting in any foundation, and has assumed the position of an authority without the research and experimental knowledge necessary to merit that title.

In spite of the good advice that has been previously tendered to him Mr. Monck is likely to go on hammering away at the Perseid radiant for a long time to come and with precisely the same negative result as hitherto. His frequent expressions, such as: "I think," "It appears to me," "I believe," etc., etc., show that he rests his argument on mere supposition and unsupported ideas and that there is no reliable groundwork whatever for his puerile criticism. Let him attempt by patient labor to accumulate facts and all the evidence possible bearing upon the subject and then he will be entitled to attention, but his present posture is certainly as ineffective as it is ridiculous.

The motion of the Perseid radiant can be observed as a conspicuous feature in July and August of every year by any one who will take the trouble to watch the paths of the meteors and record them accurately. Mr. Monck could readily witness it for himself were his observational powers up to mediocrity, but most unfortunately they are not and he has fallen into the error of supposing that mere cavilling can supply the place of practical investigation.

While, during the last quarter of a century or so, Mr. Monck has been snugly located in his feather bed and utterly oblivious as to what has been going on in the heavens, some of us have been out of doors all night watching the fall of meteors and often enough shivering with the cold. It can be fairly said of the observations which have accumulated that they were conducted with all the care and pleasurable interest such as only an intense love for the subject could inspire. The results have been published and in respect to the mobile radiation of the Perseids theory has demonstrated its perfect consistency and cut away the ground from under the feet of skeptics. But it is really quite unnecessary to recapitulate details or to enter into lengthy argument in any vain attempt to force the truth upon one to whom it appears to be so singularly indigestible.

Bristol, 1894, May 17.

W. F. DENNING.

Mathematics and Astronomy in the University of Chicago.—But for lack of space we would gladly publish the entire programme of the departments of Mathematics and Astronomy now in use at the University of Chicago. It is an excellent one and it will certainly be liberally patronized. For the immediate information of those interested, we give the

CONSPECTUS OF COURSES.—1894-5.

SUMMER.	AUTUMN.	WINTER.	SPRING.
25 Gauss's Method (See) DM	1 Astronomical Photography (Hale) DM	2 Solar Physics (Hale) 2DM	2 Solar Physics (continued) (Hale) 2DM
26 Theory of Attractions (See) DM	3 Stellar Spectroscopy (Hale) DM	35 Theory of Tides (continued) (See) DM	4 Astro-Physical Research (Hale)
27 General Astronomy (Laves) DM	30 Theory of Tides (See) DM.	36 General Astronomy (See) DM	40 Tidal Friction and Cosmogony (See) DM
28 Latitude and Longitude (Laves) DM	31 Gravitation (See) DM.	37 Dynamics of a System (Laves) DM	41 General Astronomy (continued) (See) DM
29 Seminar (See and Laves)	32 Partial Differential Equations (Laves) DM	38 Spherical Astronomy Part II (Laves) DM	42 Rotating Body (Laves) DM
	33 Spherical and Practical Astronomy Part I (Laves) DM	39 Seminar (See and Laves)	43 Special Perturbations (Laves) DM
	34 Seminar (See and Laves)		44 Seminar (See and Laves)

United States Naval Observatory.—On the 9th of June, Senator Morrill of Vermont offered an amendment to the bill before the Senate of the United States making appropriations for the naval service and for other purposes. That amendment was as follows:

That from and after the passage of this act the Superintendent of the United States Naval Observatory shall be a person selected from civil life learned in the science of astronomy, to be appointed by the President, by and with the advice and consent of the Senate, and shall receive an annual salary of five thousand dollars; he may also occupy the dwelling house near the Observatory free of rent. The Superintendent aforesaid is hereby authorized and directed, with the approval of the Secretary of the Navy, to reorganize said Observatory establishment and to make such regulations as may be expedient in relation to the Observatory and its subordinate officers, professors and other employes: *Provided*, That after January first, eighteen hundred and ninety-five, the total salaries and annual expenditures shall be adjusted to a basis of not exceeding fifty thousand dollars per annum.

On June 11 Mr. Morrill supported this amendment by a speech about ten minutes long setting forth in a clear, able, and most convincing manner the desirability of the changes proposed. He also asked that certain important papers relating to the amendment be printed to accompany it. These papers were:

1. Budget of the Royal Observatory, Greenwich, with corresponding appropriations for the support of the Naval Observatory, Washington.
2. Statement of ordinary work of the Observatory.
3. Letter from Professor Asaph Hall, in 1892, to Senator Hale of Maine.
4. Letters from S. C. Chandler, C. A. Young, B. A. Gould, Lewis Boss.
5. Extract from the Report of the Secretary of the Navy for 1892, p. 57, Naval Observatory.
6. Royal Astronomical Society of London.

Mr. Morrill's measure was favorably and unanimously reported upon by the Senate Committee on Naval affairs, and the amendment was inserted in the Leg-

islative bill by the Senate committee on appropriations. While the bill was pending in the Senate the committee consulted the Secretary of the Navy who suggested some changes. When the measure was put into shape, it proved to be one leaving the Naval Superintendent in charge, and adding to the organization a "Director of Astronomy" to have charge of the Astronomical work and to control the astronomers, assistant astronomers and computers, but nothing further. It was, however, put in by the Senate, and went back to the House. Here Mr. Reed of Maine, made a strong speech against the measure because it left the naval officers in control (See Cong. Rec., July 20). July 24, a conference committee refused to adopt the Senate measure, and so the whole matter is postponed until next session. The friends of the Morrill measure will doubtless give this matter early attention at the next session of Congress.

The Chicago Academy of Sciences.—*Section of Astronomy and Mathematics, June 11.*—The regular monthly meeting was held in the apartments of the Commerce Club, Auditorium Building; Professor G. W. Hough, President, in the chair.

Professor S. W. Burnham read the first paper of the evening on "*Astronomical Photography*." The speaker exhibited on the screen a series of the finest recent astronomical photographs, and discussed at some length the difficulties and limitations of modern photography as applied to the heavenly bodies. The photographs projected with the lantern included the celebrated photographs of the Milky Way by Professor Barnard and Dr. Max Wolf; pictures of the Orion, Andromeda and other nebulae; also photographs of several clusters, including the Pleiades, and especially Omega Centauri, as taken by Gill and Pickering. Professor Burnham then discussed the application of photography to comets, and exhibited Professor's Barnard's fine photographs of comet Swift, and his very recent photographs of comet Gale. It was remarked that photography gave the only means of obtaining an unprejudiced record of the appearance of comets, and the sudden changes which their tails undergo from day to day. At the conclusion of Professor Burnham's paper, a general discussion followed, in which all the resident astronomers took part. Dr. T. J. J. See read the second paper of the evening, on "*The determination of the relative motion of the companion of a Binary star in the line of sight, and on an approximate method of finding the eccentricities of the orbits of Spectroscopic Binaries.*" The speaker referred to his paper in the *ASTRONOMY AND ASTRO-PHYSICS* for November, 1893, in which he gave approximate formulæ for the motion in the line of sight when the orbit is not very eccentric; he then gave the rigorous formulæ for the motion in the line of sight whatever be the eccentricity. Here he supposed the elements to be known from micrometrical measures. The converse problem of finding the elements from the observed motion in the line of sight was then taken up; and the speaker showed how this problem could be solved by successive approximations. The study of the velocity curves would give the principal elements and especially the eccentricity. Dr. See referred to the important work of Pickering and Vogel on Spectroscopic Binaries, and expressed the hope that they would deduce the elements of the systems already discovered. He said that the study of these rapid systems was of great importance, as it might in a few years reveal sensible changes in the period due to the working of tidal friction. The speaker also pointed out the high importance of applying the spectroscope to the variable stars, for the purpose of deciding whether the changes are due to revolving bodies which are highly inclined upon our visual ray, as is certainly the case with variables of the Algol type.

In the discussion, Professor Hough remarked that while the theory advanced in the paper was sound, it would be difficult to actually apply it in the present state of astronomy, owing to the great delicacy required in the observations. After some further remarks the section adjourned. 6.

Disk of Jupiter's First Satellite.—The table on page 424 of our June issue is in error. In the fourth column, near the bottom, D should be inserted between the two Ps, causing all the letters above it to appear one line higher. That will credit Professor Pickering (P) with observations 1, 3, 5, 7, 9, and 11 as appears in the proof he read. The mistake was made by the carelessness of the printer.

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